

# Psychological Review

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*Harvard University*

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# THE PSYCHOLOGICAL REVIEW

## THE EFFECTS OF SOCIAL STIMULI ON THE RESPONSE TO STRESS<sup>1</sup>

EVERETT W. BOVARD

*Montreal Neurological Institute*

This paper concerns the effects of present experience, particularly social stimuli, on the response to stress. A previous paper (Bovard, 1958) concerned the effects of early experience, particularly handling, on this response.

Noxious stimuli, such as radiation, surgical injury and extreme cold, have been shown to induce a systemic, non-specific response in the vertebrate organism (Selye, 1950). This response involves release of adrenocorticotrophic hormone (ACTH) from the anterior pituitary and the consequent release of adrenal cortical hormones (cortisone, 17-hydroxycorticosterone) into the blood stream. Their effects are in general protein catabolic, involving conversion of protein into glucose, and specifically include maintenance of blood pressure and blood sugar (glucose) levels under shock. This non-specific response therefore has initial survival value for the organism under severe and sustained physical stress, but if sufficiently prolonged it will lead to cardiovascular and renal damage, and eventually to the death of the animal.

<sup>1</sup> This paper was completed during tenure of a United States Public Health Service special research fellowship, sponsored by the National Institute of Mental Health at Montreal Neurological Institute.

But psychological stress, such as immobilization for the rat, or failure to meet level of aspiration for the human (Berkeley, 1952) will also produce a pituitary-adrenal response, stimulating carbohydrate metabolism and protein breakdown. Emotional excitement, as Selye (1950; Selye & Fortier, 1950) has pointed out, is a particularly effective stress agent since it is mediated by the diffuse nerve-net of the autonomic nervous system and therefore affects wide reaches of the organism.

The important difference between psychological and physical stress is that the former does not directly affect body cells and, hence, must be mediated by the central nervous system. Transplanted to the animal's eye or temporal lobe, the anterior pituitary responds with ACTH release to so-called systemic stimuli, such as cold, adrenaline, or histamine, but not to nervous or emotional stimuli, such as intense sound or immobilization, which apparently require the hypothalamic-hypophyseal pathway (Fortier, 1951).<sup>2</sup>

<sup>2</sup> The differential response of the "denervated" anterior pituitary to different types of stress stimuli, found by Fortier (1951), has been further confirmed, in general, by experiments involving section of the pituitary stalk to the hypothalamus and other work involving destruction of the median eminence

Hence, when the terms "psychological" or "emotional" stress are used in this paper, the reference is only to the fact that under such stress the noxious stimulus must necessarily be mediated by the central nervous system. Immobilization is termed a psychological stress for the reason that it has been experimentally demonstrated that this particular noxious stimulus requires mediation of the central nervous system in order to initiate release of ACTH from the anterior pituitary.

Whether the pituitary-adrenal response to psychological (nonphysical) stress has survival value for the organism can be questioned. Experiments by Bartlett and others (Bartlett, Bohr, Helmendach, Foster, & Miller, 1954; Bartlett, Helmendach, & Bohr, 1953; Bartlett, Helmendach, & Inman, 1954) showed that animals subjected to emotional stress (restraint) and extreme cold were unable to maintain body temperature, which fell almost as rapidly as that of dead animals, whereas animals subjected to cold alone could maintain temperature reasonably well. In short, it would appear that emotional stress or excitement has an additive effect (Lavenda, Bartlett, & Kennedy, 1956), accelerating depletion of the body's vital protein and carbohydrate stores when the organism is already under physical stress.

To put this another way, then, inhibition or dampening of emotional

(Harris, 1956). The only discrepancy between the two sets of findings is the status of cold, which is classified as a systemic stress by Fortier (1951) and as a neural stress by Harris (1956). This is, however, understandable, since the response to cold includes intense activity of the sympathetic nervous system. The basic question is whether stress exists that must necessarily be mediated by the central nervous system, and the results are conclusive that emotional stresses like restraint or intense sound are in this category.

excitement component of the response to physical stress should have survival value for the organism. Relevant here are experiments showing the protective effects of sympathetic blocking agents in tumbling shock (Ross & Herczeg, 1956), tourniquet and burn shock (Millican & Stohman, 1956), and under oxygen at high pressure (Johnson & Bean, 1957).

In any case, it is hard to conceive how mobilization of carbohydrate resources, involved in the pituitary-adrenal response, helps the graduate student to meet the stress of his doctoral qualifying exams, or the lover to resolve a conflict between impulse and conscience. In the case of psychological stress, as Wolff (1953) has pointed out, the organism responds to psychological assault as if it were physical.

Thus an essential difference between psychological and physical stress is that the former, necessarily mediated by the CNS, has no invariant effect. Whether a given psychological stimulus (making a public speech) triggers a protein catabolic response is a function of the previous conditioning and present level of reactivity of the nervous system. The effect of purely physical stress, such as cold, can be considered far more invariant and unavoidable for both rat and man.

Since the pituitary-adrenal response to psychological stress can be considered maladaptive, the fact that the latter has to be mediated by the CNS permits us to intervene and prevent triggering of this protein catabolic response, by altering activity of the nervous system. From a psychological point of view, this means influencing the early and present experience of the organism. We are concerned here with the effects of present experience.



A number of studies have suggested the supportive effect of the small group under stress. Separation from the family and evacuation from London appeared more stressful for London children than did enduring the blitz with their family (Titmuss, 1950). Combat studies have suggested the effectiveness of the small group (platoon, bomber crew) in sustaining members under severe battle stress (Mandlebaum, 1952). Marshall (1951) found that battle stragglers during a retreat were relatively ineffective when put back into the line with new units, but that units that had been able to stay together fought courageously and well when put back into the line as units. Research at Boston Psychopathic Hospital (1955) has shown that lysergic acid diethylamide (LSD) taken in a group situation results in less anxiety, interpersonal distortion, and inappropriate behavior than when taken individually. These and other studies suggest that the presence of others, particularly others with whom one has previously interacted, has a protective effect under stress.

A similar effect has been found at the animal level. Liddell (1950) found that a young goat isolated in an experimental chamber and subjected to a monotonous conditioning stimulus will develop traumatic signs of experimental neurosis, while its twin in an adjoining chamber and subjected to the same stimulus, but with the mother goat present, will not. An experiment by Davitz and Mason (1955) showed that the presence of an unafraid rat significantly reduces fear response of the rat, as measured by activity in an open field. A recent experiment by Conger, Sawrey, and Turrell (1957) has shown that rats in a chronic approach-avoidance conflict situation alone had significantly greater resultant ulceration than animals tested to-

gether. On the other hand, in preliminary studies Brady<sup>3</sup> has found no evidence for any effect of the presence or absence of another monkey on the physiological damage produced in the monkey by severe emotional stress (continuous bar pressing to avoid electric shock).

Taken together, these studies at the human and animal levels suggest presence of another animal of the same species has a protective effect under stress.

An analysis of the above situations, such as the goat experiment, suggests the presence of two elements. One is a noxious agent, in these cases of a psychological nature—in that its effects (presumably, even those of LSD) are mediated by the nervous system. The other is a social stimulus, the presence of an animal of the same species.

Now a simple hypothesis to account for the observed effects is that presence of an appropriate social stimulus in the same environment as a psychological stress agent, calls forth in the organism a "competing response" which inhibits, masks, or screens the stress stimulus, such that the latter has a minimal effect. Thus the goat in the presence of its mother presumably heard the noxious conditioning stimulus, but the response to the mother was such that it masked or inhibited the stress stimulus.

Previous research has suggested that the pituitary-adrenal response to stress is initiated, in the general case, by excitation of the posterior hypothalamus, in particular by the release of a chemotransmitter into the portal veins, which carry it to the anterior pituitary.<sup>4</sup> The response of the posterior hypothalamus can be considered an essential

<sup>3</sup> J. V. Brady. Personal communication. 1959. See also Brady (1958).

<sup>4</sup> For a more extended discussion of these points, see Bovard (1958).

factor in psychological or emotional stress, since it initiates both pituitary-adrenal cortical and sympathetic-adrenal medullary activity. In general, then, the posterior hypothalamus can be considered the final common pathway for the neurohormonal influences that mediate effects of noxious physical or psychological stimuli on the organism. It would be safe to argue that physical and psychological stress cannot be distinguished once the posterior hypothalamus has been triggered.

Assume for a moment that electrical activity in the posterior hypothalamus, particularly in the region of the median eminence and mammillary bodies, has been dampened. An incoming pattern of neural impulses (the psychological stress) would therefore be less likely to trigger release of the transmitter substance from the neurosecretory cells and so initiate the entire protein catabolic sequence.<sup>5</sup>

Consider another case, where the level of activity is higher in the posterior hypothalamus. In this case, the same given incoming pattern of neural impulses would be more likely to trigger a pituitary-adrenal response.

This can all be put another way: the probability of a neutral environmental stimulus triggering the pituitary-adrenal response is a function of the level of activity in those posterior hypothalamic centers that initiate this response.<sup>6</sup>

<sup>5</sup> The mechanism here could involve lower postsynaptic potentials (Grundfest, 1957) and, hence, relative inexcitability of posterior hypothalamic neurosecretory cells.

<sup>6</sup> This argument applies equally well to the sympathetico-adrenal medulla response to stress, also mediated by the posterior hypothalamus. Both the pituitary-adrenal cortex and the sympathetico-adrenal medulla responses occur together (Ramey & Goldstein, 1957), and may in fact be considered the two arms of a single response to stress (Cleghorn, 1953). While

In psychological terms, a person or animal with a low stress threshold (high posterior hypothalamic activity) would respond to almost any environmental change with a protein catabolic response; while a person with a high stress threshold would react very seldom to environmental disturbances with a pituitary-adrenal response.

This point is nicely illustrated in a study reported by Graham (1953). Hospitalized patients were classified as mildly, moderately, or severely anxious and given equivalent isotonic saline injections. Mildly anxious patients gave the same adrenal cortical response as normal Ss; moderately anxious, a medium response; but those rated severely anxious gave the same response to saline as was found in normal Ss from an intramuscular injection of 10 to 15 mg. of ACTH. The pituitary-adrenal response of the Ss to the same stimulus was therefore a direct function of their state of "apprehensiveness," i.e., activity of the brain centers mediating fear and anxiety.

In this connection, Hess (1954) found electrical stimulation of certain points in the posterior hypothalamus

our argument, for the sake of clarity, has been confined chiefly to the pituitary-adrenal response to stress, and resultant adrenal cortical hormones, it should be kept in mind that stress simultaneously induces the release of epinephrine and nor-epinephrine (adrenaline and nor-adrenaline) into the blood stream from the adrenal medulla. The medulla is innervated by the splanchnic nerve of the sympathetic system, which is under control of the posterior hypothalamus. There has been a good deal of evidence (Goldstein, Ramey, Fritz, & Levine, 1952; Johnson & Bean, 1957; Millican & Stohman, 1956; Ross & Herczeg, 1956) to suggest that in short-term physical stress at least, sympathetic hyperactivity is the villain of the piece and that blocking of its effects, through sympathectomy or chemical agents, has survival value for the organism.

produced a fear response in the cat, motor activity passing over to flight. Stimulation of other points in this region, particularly near the descending fornix, produced a full-blown defensive reaction. Lilly (1957) elicited in monkey a state of anxiety and terror, through stimulation of the hypothalamus, just above the pituitary stalk, that was more unpleasant even than severe pain for the animal. There seems little doubt that the hypothalamus must be included in the brain centers mediating fear and anxiety.

A recent study (de Molina & Hunsperger, 1959) has shown that elaboration of elementary affective reactions is the function of an entire subcortical system, extending along the fibres of the stria terminalis from their origin in the amygdaloid nucleus (see Boward, 1958, Fig. 1) to their projections on the preoptic and perifornical zones (Hess, 1954) of the hypothalamus. Integrated defensive patterns, including growling and hissing, were elicited in cat by electrical stimulation along this pathway. This evidence suggests, therefore, that the hypothalamus is only one sector of a well-defined subcortical system for the elaboration of fear and rage patterns.

If we now assume that the presence of another person in the human studies, and presence of an animal of the same species in the animal studies, has dampened activity of the posterior hypothalamus, the observed phenomena can be accounted for. Not only would the pituitary-adrenal reaction be dampened in such a case, but sympathetico-adrenal medulla activity also would be inhibited. Defensive (fear and rage) responses, in elaboration of which the posterior hypothalamus has a part, would also be reduced, it can be noted here, though our primary concern in this paper is with the neuroendocrine

rather than with the behavioral responses to noxious stimuli.

A number of recent studies (Gellhorn, Nakao, & Redgate, 1956; Shimazu, Okada, Ban, & Kurotsu, 1954) have suggested a reciprocal inhibitory effect between the posterior hypothalamus, concerned with protein catabolism in long- and short-range responses, and the anterior hypothalamus and parasympathetic centers, concerned largely with anabolic functions. Stimulation of the latter region would appear to inhibit activity of the former.<sup>7</sup>

The simplest hypothesis to account for the observed phenomena at the human and animal levels is, therefore, that the presence of another member of the same species stimulates activity of the anterior hypothalamus and thus, as a by-product, inhibits activity of the posterior hypothalamus and its centers mediating the neuroendocrine response to stress. Previous interaction with the other person or animal, as the case may be, could be assumed to accentuate this effect.

Experimental testing of the proposition that social stimuli dampen the response to stress could involve, as indices of this response, either the pituitary-adrenal cortical or the sympathetico-adrenal medullary components (see Footnote 6). Both responses occur together and are mediated by the posterior hypothalamus.

*Sympathetico-adrenal medulla.* The reaction of the sympathetic division of the autonomic nervous system in stress is important to our argument, not only because this system reacts as a unit

<sup>7</sup> The reciprocal relation of anterior and posterior hypothalamus appears to be reflected in the inverse relation (Selye, 1950, p. 103) of growth hormone (somatotrophin or STH) production, mediated by the anterior hypothalamus, and ACTH production, under stress.

and affects wide reaches of the organism and can be held responsible for short-term damage under psychological stress, but because it is much more accessible to experimentation by psychologists than the pituitary-adrenal reaction.

Heart rate has been used by a number of investigators as an index of sympathetic activity. In an experiment with human Ss, Notterman, Schoenfeld, and Bersh (1952) found the unconditioned response to electric shock was acceleration of heart rate.

Two other measures show promise as indices of the response to emotional stress. A study by Newton, Paul, and Bovard (1957) showed a drop of 4.0 degrees F. in finger temperature of Ss who had just experienced a three-minute role-playing situation in which they were "fired," compared to a drop of .6 degrees for Ss who had just experienced a situation in which they were "promoted." A recent study by de Long, Uhley, and Friedman (1959) has shown that exposure to psychological stress (periodically and alternatively electrically charging one half of the floor of a specially constructed cage, and so forcing migration from one side to the other), produced in the laboratory rat a significant reduction in blood clotting time, roughly proportional to the period of stress. The connection of blood clotting time to sympathetic reactivity, however, has not yet been established.

A direct measure of sympathico-adrenal medulla activity would, of course, be chemical analysis of urine for epinephrine and nor-epinephrine content.

In these experiments, whatever the measure used for sympathetic response, the essential design would be to examine the physiological reaction of the

S to stress under two conditions: alone, and in the presence of one or more members of the same species with whom the S has previously interacted. A particularly effective psychological stress for human Ss would be the role-playing situation mentioned above; for the laboratory rat, a brief period of restriction in a plastic box with air holes, just big enough for him to fit in comfortably, has an immediate and pronounced effect. Incidentally, sympathetic activity in the rat is quickly demonstrated to the handler by cooling of the rat's paws. A convenient measure of this animal's reaction to emotional stress would be, therefore, paw-temperature, measured by a thermocouple as is done for finger temperature in human Ss.

In all these studies, the basic hypothesis would be that, in the presence of a social stimulus, the S's physiological response to stress would be significantly reduced.

#### *Pituitary-adrenal cortical response.*

Measurement of this response, while more difficult for those with psychological training only than measurement of the sympathico-adrenal medulla reaction, is nevertheless more important theoretically and is the very heart of the matter for the argument presented in this paper. Therefore if the necessary liaison can be established with a biochemist or two, direct measurement of this response would be worthwhile for the experimenter.

Previous work with human Ss (Persky, Hamburg, Basowitz, Grinker, Sabshin, Korchin, Herz, Board, & Heath, 1958) and with monkeys (Mason, Harwood, & Rosenthal, 1957) has shown that blood plasma content of an adrenal cortical steroid, 17-hydroxycorticosterone (hydrocortisone), can be used in these species as an index of

the response to emotional stress. In a new method recently developed for the rat, adrenal steroid output under stress can be conveniently measured (Guillemin, Clayton, Smith, & Lipscomb, 1958). While these direct measures are best for scientific purposes, they are inconvenient in the case of human Ss, since blood samples must be taken for analysis.

Now the reader may have noted a different taste in his saliva under intense emotional stress. This is a result of a change in electrolyte balance, i.e., the ratio of potassium to sodium, in the saliva as a result of pituitary adrenal activity under stress. A series of preliminary experiments by Grad<sup>\*</sup> has suggested that the potassium-sodium ratio in saliva can be used as a measure of response to emotional stress (talking to the experimenter while wearing glasses that exclude all light), in human Ss. The saliva required can be conveniently obtained and the chemical analysis required is not too complicated for a psychologist without any special training in chemistry.

In these experiments, the aim would be to examine the result of stress on blood and saliva content in the presence and absence of an appropriate social stimulus.

Another method of getting at the effect of social stimuli on the response to stress is suggested by the experiments of Bartlett et al. (1953, 1954) referred to above. It can be hypothesized that body temperature under extreme cold or heat can be better maintained in a group of persons with a previous history of interaction, compared to a group without such experience, since the emotional component of

these stress agents would be reduced or eliminated for Ss in the first group.

*Anterior hypothalamus.* To account for the dampening of the response to stress by social stimuli, it has been suggested that social stimuli activate the anterior (parasympathetic) hypothalamus, this increased activity in turn inhibiting the activity of the posterior hypothalamus. While reciprocal inhibition of the anterior and posterior hypothalamus has been experimentally demonstrated, as noted above, there is as yet no evidence for the hypothesis that social stimuli activate the anterior hypothalamus.

The work of Redgate and Gellhorn (1955) suggests this hypothesis could be tested by examining blood pressure and pulse rate of the S under a social stimulus (no stress need be present in this experiment). If anterior (parasympathetic) hypothalamic activity is increased under the social stimulus, then the S should show a slower return of blood pressure from drug-induced hypotension and an increase in reflex slowing of the pulse from nor-epinephrine, compared to a control situation in which the social stimulus was absent.

A crucial point in this discussion has been whether, when the posterior hypothalamus is activated under stress, a "competing response" to social stimuli occurs in the anterior hypothalamus, thus partially inhibiting posterior hypothalamic activity. Putting the matter this way assumes that neural patterns initiated by the noxious stimulus arrive at the posterior hypothalamus, while neural patterns initiated by the social stimulus arrive at the anterior hypothalamus. But this assumes that identification of each type of stimulus and routing of the resultant neural impulse has already taken place: hypothalamic

<sup>\*</sup> B. Grad. Personal communication. 1958.

activity is the result, so to speak, of decisions taken elsewhere in the nervous system as to the nature of the stimulus.

What is therefore required, is a non-specific modulation of hypothalamic activity, determining relative levels of anterior and posterior activity, and a means of identifying environmental stimuli as noxious, reinforcing, or neutral. The amygdaloid complex (see Fig. 1, Bovard, 1958), a secondary olfactory center in the temporal lobe, with its rich connections to both anterior and posterior hypothalamus, can provide just such nonspecific modulation of hypothalamic activity (Gloor: 1955a, 1955b), thereby in effect setting the threshold for stress. Further, in its absence, some animals have been found to have difficulty in identifying motivationally relevant stimuli (food, natural enemies) as noxious or reinforcing.

Experimental evidence on the role of the amygdaloid complex in the response to stress is accumulating, and will be discussed at length elsewhere (see also Bovard, 1958). For the present, it seems a likely assumption that the effects of social stimuli on the response to stress are mediated by the amygdaloid nucleus.

The hypothesis that social stimuli dampen the response to stress would seem to have implications for fields as widely different as psychotherapy and interplanetary exploration.

It could be expected that one of the effects of the personal relationship found by Fiedler (1950) and others to be an essential factor in psychotherapy would be to help free the patient (either in group or individual therapy) from the stress reaction his own ideas and impulses would ordinarily trigger,

by raising his over-all stress threshold. It is perhaps not too much to suggest that the physician-patient relationship, and its effects on the course of an illness, can be in part understood in terms of the resultant dampening of the emotional stress component of the patient's illness, hence leaving him more of his metabolic resources in reserve to deal with the physical onslaught of disease.

Further, anesthetists have found that visiting the patient the night before an operation and establishing a personal relationship with him has value in inducing quick and easy anesthesia on the operating table the next day. In general, it has been found in medical practice that the more frightened and upset the patient is, the more difficult it is to induce anesthesia; that is to say, the more active the sympathetic system, the more resistance the anesthetic drug has to overcome in quietening the patient.<sup>9</sup> Thus the amount of anesthetic required in the operation is in part a function of the personal relationship between the patient and his physicians.

In recent work with rats, the writer has observed that the more emotionally reactive the animal is prior to operation, the larger the dose of anesthetic required to put him to sleep. Criteria for unconsciousness: when hard pinches of the tail and hind paws no longer elicit squeals.

In interplanetary exploration, where sudden changes of environment could be expected (from the results of animal experiments) to produce a severe pituitary-adrenal stress response, this reaction could be held to a minimum by proper use of social stimuli. The effect of presence or absence of others

<sup>9</sup> P. Gloor. Personal communication. 1959.



(and amount of previous interaction with them) on the S's response to stress could be determined in isolation experiments, using sudden and pronounced changes in the internal laboratory environment as the stress and measuring the result in terms of adrenal steroid blood plasma content, or potassium-sodium ratio in saliva and blood coagulation time if these latter are found to be reliable indices of the response to stress. Substitute social stimuli, such as sound of the human voice on tape, radio broadcast, or direct radio telephone communication also could be tested to determine their relative effectiveness in dampening the stress reaction for those space pioneers who must make it alone.

By using social stimuli to dampen the response to psychological stress of space personnel, and by selecting personnel who have high thresholds for stress to begin with, a minimal response to the stress of space exploration could be obtained.

At a more mundane level, it can be suggested our understanding of the stress response may one day be expressed in the form of institutionalization of those forms of interpersonal relationship that, in early and present experience, can be shown to increase the individual's tolerance for psychological stress.

The presence of, and interaction with, another member of the same species has been shown to have a protective effect on the vertebrate organism under stress. This effect is accounted for in terms of dampening of the pituitary-adrenal cortical and sympathetico-adrenal medullary responses to stress, through inhibition of the posterior hypothalamic centers that, in the general case, trigger these reactions. Such inhibition is hypothesized to result

from stimulation of anterior hypothalamic activity by the social stimulus.

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## MATHEMATICAL THEORY OF CONCEPT IDENTIFICATION<sup>1</sup>

LYLE E. BOURNE, JR.

*University of Utah*

AND FRANK RESTLE

*Michigan State University*

In the experiments to be discussed in this paper, a variety of concept-identification problems have been constructed and tested. It has been found that variations of the sequence of stimuli and the conditions of reinforcement result in systematic variations in the performance of college students. The over-all result of these studies is a complex network of functional relationships. The purpose of this paper is to show that a few assumptions about learning, already tested in other contexts, can account for most of the results in quantitative detail.

We first describe the general experimental procedures and show how these procedures are to be interpreted theoretically. Next we state the difference equations of the theory and derive the computing formulas to be used. Our tests will center on predicting the total errors made in solving each problem, since this is a natural and stable statistic, and one which can be predicted conveniently from the theory used. Finally we discuss five groups of experiments, giving a statement of our quantitative predictions and a report of the data and calculations which test the theory.

The concept identification task

studied in our experiments has been described completely in earlier reports (e.g., Bourne, 1957). By means of a stripfilm projector, geometric patterns are presented one at a time to *S*. These patterns are combinations of *R* relevant and *I* irrelevant binary stimulus dimensions. The *S* responds to each pattern by pressing one of a number of available keys to identify the category to which the pattern belongs. After each response the correct answer is signaled automatically.

Both two- and four-category problems are used. In a two-choice problem, a typical solution would be effected by putting all red patterns in one category and all green patterns in the other. In this case we say that the color dimension is relevant. If it should happen that all the red patterns are triangles and all the green ones are squares, then the problem could be solved either by the color or by the shape dimension. We say that both color and shape are relevant and they are *redundant*. If some patterns are large and some small, but both large and small patterns appear in Category 1 and both large and small appear in Category 2, we say that the size dimension is irrelevant.

In a four-choice problem there are two *independent* relevant dimensions. For example, Category 1 may contain all red squares, Category 2 all red triangles, Category 3 all green squares, and Category 4 all green triangles. In this case both color and size are relevant but they are independent.

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If all four categories contain both large and small patterns, we say that size is irrelevant.

The interpretation of the two-choice problem is essentially that used in other analyses based on the present theory (e.g., Restle, 1959). The set of relevant cues, in a problem with several redundant relevant dimensions, is the union of the sets that belong with each such dimension. We assume in applications here that all sets are disjoint, so that the measure of the union of several sets of cues is the sum of their separate measures. A set of irrelevant cues is associated with each irrelevant dimension. Another set of irrelevant cues, of unspecified source, is introduced to take account of internal stimuli and incidental distractions which may affect performance.

The experimental procedure determines whether a given set of cues is relevant or irrelevant. The measure of a set of cues, e.g., the measure of the set of color cues, must be estimated from the data. It was known from earlier methodological studies that the various dimensions of physical variation used in the present experiments would produce roughly equal amounts of cues. Thus in our calculations we assume that the measure of relevant cues is proportional to the number of relevant dimensions, and the measure of irrelevant cues from dimensions alone is proportional to the number of dimensions made irrelevant.

Our method of deciding whether or not a dimension gives rise to a set of cues depends on the nature of the sequence of patterns. Any geometric form used in our experiments must have some color, some shape, some size, some location, etc. This would suggest that color, shape, size, and location cues must all be present in

every problem. However, if all forms in an experimental series are the same shade of red, it seems to us unlikely that color would have any appreciable effect on behavior. When we wish to remove a set of cues from consideration, that "dimension" is held constant throughout the sequence of stimulus patterns. If both values of the dimension occur, as when both red and green forms are shown on different trials, the color cues are assumed to be present. They are relevant if one response is correct to red forms and another response to green forms. They are irrelevant if the correct response is uncorrelated with color.

In our theory, the main parameter, or structural property of the problem, is the proportion of relevant cues. However, this does not mean that a mere physical inspection of the situation enables us to predict performance, because cues arise from the patterns *as the patterns are received and evaluated by S*. Rats, monkeys, and men do not get the same cues from the same physical situation; we cannot even expect all organisms of the same species to get the same cues. Problems and dimensions must be calibrated, using some of the learning data, to find out what measure of cues corresponds to each dimension. Once this calibration is completed for a class of *Ss*, the dimensions may be recombined in various ways to produce new problems; the theory makes specific quantitative predictions of the curve of learning for many such recombinations.

The four-choice problems used in several experiments require a new interpretation within the theory. These problems require *S* to perform two binary discriminations simultaneously to get the correct response. For example, if the four categories are red-

square, red-triangle, green-square, and green-triangle, *S* must respond correctly on both the color and the shape dimensions to make the correct choice. Even if *S* should correctly guess that color and shape are to be the relevant dimensions, he can assign them to the wrong response dimensions. In the example stated, he might decide between Keys 1, 2 versus 3, 4 on the basis of shape and 1, 3 versus 2, 4 on the basis of color, when the opposite arrangement is in fact correct. Initially, the two arrangements are equally possible. We assume that *S* learns nothing to the degree that he entertains the wrong hypothesis about response dimensions and learns at the rate appropriate for the two-choice problem if he takes the correct hypothesis. The two ways of arranging stimulus and response dimensions can each be assigned a priori probabilities of one-half.

In some of our experiments the delay of reinforcement (information feedback) is varied. In all cases, as soon as *S* presses a key the stimulus pattern disappears. In the usual procedure, the reinforcing light over one of the keys is lit immediately. It is our interpretation that *S* learns the contingencies between patterns and reinforcing lights. A delay interval between these events requires that the connection be formed between a trace of the pattern and the reinforcing light. In this situation, delay of reinforcement should inhibit learning because the traces undergo random decay or fluctuation during the delay period, and *S* can learn only those cues which survive the delay.

The above discussion will be formalized next in a set of difference equations. The mathematical structure is essentially that developed in earlier papers by the second author (Restle: 1955, 1957a, 1958), but it has been

specialized and amended in the light of the interpretations mentioned above.

#### MATHEMATICAL MODEL

Mathematically, a concept identification problem is described as a set of cues. Cues arise from variations of stimulus dimensions as discussed above or from sources, such as background irrelevant cues from the apparatus, from the surround, or from within *S*. The intention is to be entirely inclusive: anything *S* can use as a basis of response is part of the total set of cues. Let this set, for a particular problem, be called *K*, and let any member of that set be called *k*, or *k'*, etc.

#### Conditioning

We let  $F(k, n)$  stand for the probability that Cue *k* is conditioned to the correct response at Trial *n*. If *k* is an irrelevant cue then we may take  $F(k, n)$  to be chance,  $\frac{1}{2}$  in a two-choice case, for all Trials *n*; no matter what response might be conditioned to an irrelevant cue, it has only a chance probability of being correct. Relevant cues are modified by reinforcement so that they are conditioned to the correct response. We employ standard linear difference equations: on trials during which reinforcement for the correct response is given,

$$F(k, n+1) = F(k, n)(1 - \theta) + \theta \quad [1]$$

and when reinforcement is omitted, since there is no learning,

$$F(k, n+1) = F(k, n) \quad [2]$$

Since the incorrect response is never reinforced in our experiments, we need not consider that case in the theory. If reinforcements are given on  $\pi$  of the trials at random, the solution to the equations is

$$\begin{aligned} \bar{F}(k, n) = & 1 \\ & - (1 - F(k, 1))(1 - \theta)^{n-1} \quad [3] \end{aligned}$$



In this equation  $\bar{F}$  is the mean proportion of relevant cues conditioned to the correct response; the mean is taken over all experimental sequences with probability of reinforcement equal to  $\pi$ . For most of our experiments, reinforcement is given each trial and  $\pi = 1$ .

#### Adaptation

In this model we suppose that irrelevant cues, which are not predictors of reinforcement, become neutralized or adapted. When a cue is adapted it is ineffective in determining response. Letting  $A(k', n)$  be the probability that an irrelevant cue  $k'$  is adapted at Trial  $n$ , we assume the linear equations

$$A(k', n+1) = A(k', n)(1-\theta) + \theta \quad [4]$$

if reinforcement is given on Trial  $n$ , and

$$A(k', n+1) = A(k', n) \quad [5]$$

if no reinforcement is given. With reinforcements given on  $\pi$  of the trials at random, the solution is

$$\bar{A}(k', n) = 1 - (1 - A(k', 1))(1 - \pi\theta)^{n-1} \quad [6]$$

Note that the rate parameter,  $\theta$ , governing conditioning (Equation 1) and adaptation (Equation 4) is the same. This is a special case of previous forms of the model, when adaptation is of completely irrelevant cues (Restle: 1957a, 1958).

#### Probability of a Correct Response

In this theory, the probability of a correct response is the proportion of functional (not adapted) cues which are conditioned to the correct response. Letting  $K$  be the set of cues in the problem,

$$p_n = \frac{\sum_K [F(k, n)(1 - A(k, n))]}{\sum_K (1 - A(k, n))} \quad [7]$$

If there is some randomness in the experimental procedure (e.g., some blank trials given at random), we have expressions only for the mean values of  $F$  and  $A$ . These may be inserted into Equation 7 if we assume that the total number of cues in  $K$  is large. This assumption is made where necessary.

Suppose that  $S$  begins naive, with  $F(k, n) = \frac{1}{2}$  for all cues and  $A(k, n) = 0$ . It can be shown that if  $r$  is the proportion of relevant cues,

$$p_n = 1 - \frac{\frac{1}{2}(1-\theta)^{n-1}}{r + (1-r)(1-\theta)^{n-1}} \quad [8]$$

(see Restle, 1955).

#### Theory of Parameters

A strong assumption of the model is that  $\theta$ , which controls the rate of learning, is equal to  $r$ , the proportion of relevant cues. In the experimental applications we interpret this assumption as follows: if  $r$  is the proportion of relevant cues present at the time of reinforcement and  $k$  is a relevant cue present at that time, then with probability  $r$  Cue  $k$  will be conditioned. If  $k'$  is an irrelevant cue present at the time of reinforcement, then with probability  $r$  Cue  $k'$  will be adapted.

In some experiments the mean probability (over trials) that a certain cue will be conditioned or adapted may be less than  $r$ . This may happen because reinforcements are omitted on some trials—no learning would occur on such a trial (Atkinson, 1956)—or because reinforcement is delayed and some cues are not present at the time of its occurrence (Burke, Estes, & Hellyer, 1954). Suppose that Cue  $k$  coincides with a reinforcement on some proportion  $\alpha$  of the trials, and on those trials the proportion of relevant cues is  $r$ . Then the mean

probability of conditioning or adaptation,  $\bar{\theta}$ , will be the joint probability that cue and reinforcement occur, and that the cue is learned on that trial if present. We assume that these two events are independent, so that we set

$$\bar{\theta} = \alpha r \quad [9]$$

#### *Theoretical Interpretation of the Four-Choice Problem*

We have interpreted the four-choice problem as requiring  $S$  to perform correctly on both of two two-choice problems. We consider one of the relevant dimensions as if it were irrelevant for learning the other dimension in computing  $r$ , the proportion of relevant cues. Furthermore, since there are two possible response dimensions and  $S$  does not know which to use for a given stimulus dimension, we suppose that the learning rate is just half what it would be in the two-choice problem. If every cue is present every trial and reinforcements are always given, we should have  $\theta = \frac{1}{2}r$ .

Then, if  $p_1(n)$  is the probability of correct response on one dimension and  $p_2(n)$  is the probability of correct response on the other, and  $p_c(n)$  is the probability of a correct response on the four-choice problem, we set  $p_c(n) = p_1(n)p_2(n)$ . In our experiments the two dimensions are about equally difficult and the  $S$ s are naive, so that to an approximation

$$p_c(n) = (p_1(n))^2 \quad [10]$$

From Equation 8 we have an expression for  $p_1(n)$  for a naive  $S$ . Then

$$\begin{aligned} p_c(n) &= \left( 1 - \frac{\frac{1}{2}(1-\theta)^{n-1}}{r + (1-r)(1-\theta)^{n-1}} \right)^2 \\ &= 1 - \frac{(1-\theta)^{n-1}}{r + (1-r)(1-\theta)^{n-1}} \\ &\quad + \frac{(1-\theta)^{2n-2}}{4(r + (1-r)(1-\theta)^{n-1})^2} \quad [11] \end{aligned}$$

where  $\theta = \frac{1}{2}r$ .

#### *Estimation of Parameters*

There are many ways to estimate the parameters of a learning model (Bush & Mosteller, 1955), but we have chosen to concentrate on one which seems natural and appropriate to our data and model. The model assumes that the probabilities of success on any two trials are independent. This can be seen by inspecting Equation 8 and Equation 11, which give the probabilities of success on Trial  $n$  in two- and four-choice problems. Note that  $p_n$  can in both cases be computed without reference to what responses may have occurred on previous trials.

It is well known that if one throws a coin  $n$  times, where the probability of a head on each toss is  $p$ , then the expected number of heads is  $np$ . It is equally true that if one tosses coins with probabilities  $p_1, p_2, \dots, p_n$  of coming up heads, the expected number of heads is  $\sum_{i=1}^n p_i$ , provided that the tosses are independent. In our situation, the expected number of successes in  $N$  trials is  $\sum_{n=1}^N p_n$ , by the same reasoning, and the expected number of errors is  $\sum (1 - p_n)$ .

Thus from Equation 8, in a two-choice problem, the expected number of errors on Trials 2 to  $N$  is

$$\begin{aligned} \bar{E}_N &= \sum_{n=2}^N (1 - p_n) \\ &= \frac{1}{2} \sum_{n=2}^N \frac{(1-\theta)^{n-1}}{r + (1-r)(1-\theta)^{n-1}} \quad [12] \end{aligned}$$

We have not found a way to evaluate this sum exactly, but a good approximation is obtained by taking

$$\begin{aligned} E_N &\cong \frac{1}{2} \int_1^N \frac{(1-\theta)^{t-1} dt}{r + (1-r)(1-\theta)^{t-1}} \\ &= \frac{1}{2} \frac{\log(r + (1-r)(1-\theta)^{N-1})}{(1-\theta) \log(1-\theta)} \quad [13] \end{aligned}$$

If training is continued until the probability of an error ( $1 - p_n$ ) is very small, the approximation becomes

$$\bar{E} \cong \frac{\frac{1}{2} \log(r)}{(1 - \theta) \log(1 - \theta)}$$

and, when  $\theta = r$  as in simple learning,

$$\bar{E} \cong \frac{\frac{1}{2} \log(\theta)}{(1 - \theta) \log(1 - \theta)} \quad [14]$$

In these calculations we have discarded the first trial previous to which no learning has taken place. This equation has been published elsewhere (Restle, 1955). Since Equation 14 is not easily solved for  $\theta$ , we have included a graph of the function in Fig. 1.

Similar steps of reasoning on Equation 11, which gives the learning curve for simple four-choice problems, also gives an equation relating  $r$  or  $\theta$  to the expected total errors. In this case, of course,  $\theta = \frac{1}{3}r$ . Since the equation is quite large we do not give it here, but we do include a graph of the function in Fig. 1.

Whenever we have a simple problem, with no missing reinforcements or stimulus trace decay to take account of, the functions plotted in Fig. 1 can be used to estimate  $\theta$  or  $r$  from the learning data expressed as mean total errors. In what follows, "estimated values of  $\theta$ " are obtained by the above methods using total error scores. In those cases where stimulus trace decay or missed reinforcements are studied, we estimate  $r$  or  $\theta$  by the graph for cases in which  $\alpha = 1$  (control groups with no delay of reinforcement, or no missing reinforcements, etc.). We then make predictions, using more general forms of the equations for total errors in which  $\alpha (\neq 1)$  appears, e.g., Equation 13 with  $\theta = \alpha r$ .

#### EXPERIMENTAL EVIDENCE

The model as stated asserts that the rate of learning depends on the pro-

portion of relevant cues and certain other factors, such as stimulus trace decay. Furthermore, we have derived equations relating the rate of learning to total error scores. If this model is correct, it means that concept-identification experiments can be used to test a limitless variety of quantitative hypotheses. We shall limit ourselves to three main hypotheses concerning: (a) the additivity of cues (Restle: 1955, 1957b, 1958, 1959), in support of which we amass evidence on the additivity of *irrelevant* cues which had not previously been demonstrated; (b) the relation between two- and four-choice problems constructed from the same dimensions; and (c) the exponential decay of stimulus traces, as would follow from the concept of random stimulus fluctuation (Estes, 1955).

#### Redundant Relevant Dimensions: Calibrating Relevant Dimensions

Our first experimental test is of the additivity of cues, discussed in

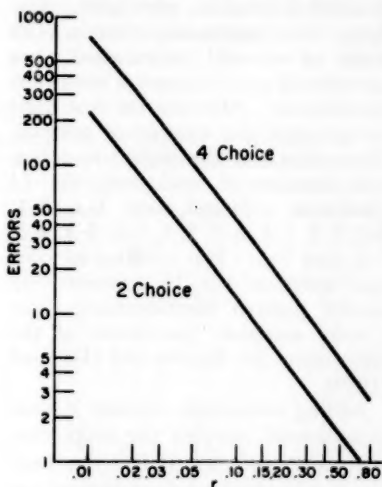


FIG. 1. Expected number of errors as a function of proportion of relevant cues, on a log-log plot. Nomographs for two- and four-choice learning.

earlier papers on this model (e.g., Restle, 1955). Various problems were constructed, all using the same apparatus, procedure, and irrelevant dimensions, but with different numbers of redundant relevant dimensions. Recall that dimensions are made relevant and redundant as follows: Suppose that a red pattern belongs to Category (button) 1 and a green pattern belongs to Category (button) 2. We can make all red patterns triangles, and all green ones squares; then *S* can solve the problem either by color or by shape. Both dimensions are relevant, and they are redundant. Pattern presentation, recording, and control of the time characteristics were automatically performed. One hundred and eighty *Ss* were assigned randomly to 12 treatment combinations as determined by an incomplete  $6 \times 3$  factorial design. Six levels of redundant relevant stimulus information, 1, 2, 3, 4, 5, and 6 dimensions, and three levels of non-redundant irrelevant information, 1, 3, and 5 dimensions, were used. The design was incomplete since not all levels of relevant information were represented at all degrees of irrelevant information. Allowing the first digit to represent the number of relevant dimensions and the second to represent number of irrelevant, the 12 conditions explored were 1-1, 2-1, 3-1, 4-1, 5-1, 6-1, 1-3, 2-3, 3-3, 4-3, 1-5, and 2-5. The criterion of concept solution was 16 consecutively correct pattern identifications. For a more complete description of the experiment, see Bourne and Haygood (1959).

Adding redundant relevant dimensions should increase the proportion of relevant cues,  $r$ . If  $R$  is the number of redundant relevant dimensions, and each contributes the same sized subset of cues, then the number of

relevant cues should be proportional to  $R$ ; it should be  $kR$ , where  $k$  is an unknown constant. Similarly, the total number of cues (relevant and irrelevant) in the situation should be  $a + bR$ , where  $a$  represents the amount of irrelevant stimulation from irrelevant dimensions and other sources, and  $bR$  is the amount of relevant and irrelevant stimulation arising from the redundant relevant dimensions. Then the proportion of relevant cues should be

$$r = \frac{kR}{a + bR}$$

From the data and the nomograph, Fig. 1, we can estimate  $r$  for various values of  $R$ . We then may determine values of  $k$ ,  $a$ , and  $b$ . It is easily seen that estimating two of these constants is enough; since we deal only with ratios, one of them can be chosen arbitrarily as the unit of measurement of subsets of cues. We let  $b = 1$  so that the total contribution of cues from a single dimension is 1. The data indicate that  $k = \frac{1}{2}$  and that  $a = 3.4 + I$ , where  $I$  is the number of irrelevant dimensions in the problem. In this case, we have

$$r = \frac{\frac{1}{2}R}{R + I + 3.4}$$

This formula states that relevant and irrelevant dimensions contribute the same amount of cues, that relevant dimensions yield half relevant and half irrelevant cues, and that other sources of irrelevant cues in the situation have an effect corresponding to about 3.4 dimensions.

These parameter values, which we shall use in later computations, were arrived at indirectly. First, for various values of  $R$  and  $I$ , we estimated  $r$  from the data, by Fig. 1. We then tried to find optimal values for  $k$  and  $a$  which would yield a set of  $r$ 's close

to those obtained. The parameters were chosen by an informal iteration and are not least-squares solutions. We sought to select parameters which would permit a good reproduction not only of the estimated  $r$ 's, but also of the raw (total errors) data; explicit solution of the needed estimation equations turned out to be difficult.

The equation above is not accurate with respect to the color dimension, though it seems satisfactory for any of the others. Apparently the color dimension is much more discriminable than the others, in our experiments, and we estimate that *all* cues from the color dimension are relevant. With this adjustment, we find that the parameters reproduce the data adequately. In Table 1, the fitted values of  $r$  (or  $\theta$ ) are compared with those estimated from the experiment; the expected error scores, fitted and obtained, are also compared. The results of this computation are shown graphically in Fig. 2. The fact that the model adequately describes the error scores of these 12 groups is significant, although this analysis

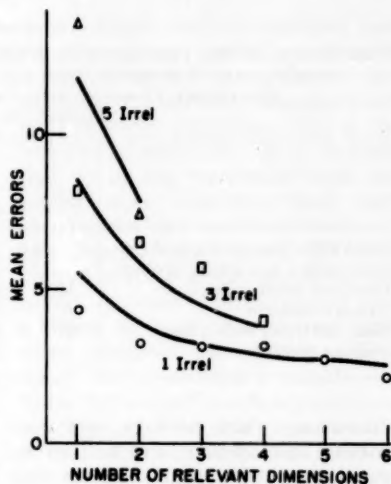


FIG. 2. Mean number of errors as a joint function of the number of redundant relevant and the number of independent irrelevant dimensions. Solid lines are theoretical.

is not presented as a precise test of the theory. More important is the fact that we now have some confidence that the various dimensions yield about equal amounts of cues, and that (except for color) each

TABLE 1  
COMPUTED AND OBTAINED  $r$  AND MEAN ERRORS AS A FUNCTION OF NUMBER OF REDUNDANT RELEVANT AND NUMBER OF INDEPENDENT IRRELEVANT DIMENSIONS

No. of Re. Dimensions	No. of Irrel. Dimensions	Computed $r$	Obtained $r$	Computed Errors	Obtained Errors
1	1	.19	.22	5.5	4.3
2	1	.24	.27	3.9	3.2
3	1	.28	.28	3.2	3.1
4	1	.30	.28	2.8	3.1
5	1	.32	.32	2.7	2.7
6	1	.34	.38	2.5	2.1
1	3	.14	.14	8.2	8.2
2	3	.18	.16	5.7	6.5
3	3	.21	.18	4.4	5.7
4	3	.24	.24	3.8	3.9
1	5	.11	.09	11.8	13.6
2	5	.14	.15	7.8	7.4

TABLE 2

COMPUTATION OF THE PROPORTION OF RELEVANT CUES ( $r$ ) IN A FOUR-CHOICE PROBLEM ASSUMING THAT DIMENSIONS HAVE THE SAME WEIGHT AS IN A CORRESPONDING TWO-CHOICE PROBLEM, WITH VARIABLE NUMBER OF REDUNDANT IRRELEVANT DIMENSIONS

	Number of Irrelevant Dimensions				
	1	2	3	4	5
Predicted $r$ , four-choice problem	.212	.220	.197	.166	.129
Estimated $r$ , four-choice problem	.222	.216	.205	.148	.135
Predicted mean errors, four-choice problem	15.6	14.8	17.0	21.0	29.8
Observed mean errors, four-choice problem	14.7	15.4	16.1	24.2	28.0

dimension, when relevant, will contribute half relevant and half irrelevant cues. We shall employ this information in predicting performance in more complicated experiments, a task to which we now turn.

#### *Two- and Four-Choice Problems*

The first use to which we put these estimates is in studying the correspondence between two- and four-choice problems. In one experiment (Bourne & Haygood, 1959), independent groups ( $N = 10$ ) of Ss were required to solve two-choice (one relevant dimension) and four-choice (two independent relevant dimensions) tasks. The experiment was replicated five times, with 1, 2, 3, 4, or 5 redundant irrelevant dimensions in the problem. The apparatus and procedure were essentially the same as in the earlier study.

From the data of the two-choice problems, we estimate that  $r = .34$ , .36, .30, .24, and .17 for the five different levels of irrelevant information. In the four-choice problems  $r$  will be lower because, with two independent relevant dimensions, each is irrelevant for learning the other. Consider the first experimental case, with  $r = .34$ . Disregarding the color

dimension for the present, we note that in theory this means

$$r = \frac{\frac{1}{2}}{1 + B} = .34$$

where  $B$  is the amount of irrelevant stimulation, and a dimension is given unit weight. In these units,  $B = .47$ . This does not correspond very well with our estimate of background cues from the previous experiments, but the difference may be due to better experimental technique.

In the four-choice problem, there will be one more dimension acting as irrelevant in learning this dimension. Then,

$$r = \frac{\frac{1}{2}}{1 + B + 1} = .20$$

whence we should expect that, in the four-choice problem,  $r = .20$ . The obtained value was .22, which is rather close.

In our detailed computations we have taken account of the color dimension whenever it appears as relevant. The predicted and estimated values of  $r$  for the four-choice problem are shown in Table 2, along with the total error scores expected from our predictions and the obtained total error scores. The pre-



dictions fall very close to the data. We emphasize that in this case we predicted the total error scores on the four-choice problems without using any four-choice data; the predictions were based only on the data from two-choice problems. Since the two- and the four-choice problems differed widely in difficulty, the ability to predict one from the other indicates that the model is quite powerful.

However, when we repeated the computations on the data from an experiment by Walker (1958), our results were not as good. It should be mentioned that Walker's stimulus patterns were drawn figures presented to *S* by a memory drum; *S* was paced through the pattern sequence at a 6-sec. rate. Also, we note that Walker's problems differed in number of *independent* irrelevant dimensions. Computations are shown in Table 3; our predictions were far from the

facts. Closer analysis revealed that the discrepancy could be explained accurately as due to one factor—the background irrelevant cues were much more powerful in the two- than in the four-choice problems. It is possible that *Ss* in the two-choice tasks expected more difficulty than they encountered and produced irrelevant hypotheses of their own. We have noticed in these simple problems that college students often invent hypotheses far more complicated than those produced by relevant dimensions. It is perhaps reasonable to think that in the four-choice problems *Ss*, faced with what is *prima facie* a difficult task, would be less likely to invent difficulties. In fact, however, we have no evidence that this, or Walker's methodological change, is the actual source of the discrepancy.

#### *Additivity of Irrelevant Dimensions*

Since we know expected total errors as a function of  $r$ , the theory tells us how total errors should vary as a function of number of irrelevant dimensions,  $I$ . Over the range of error scores likely to be encountered in the laboratory (up to about 1,000 errors), expected total errors is a slightly sigmoid, almost linear function of  $I$ . This theoretical deduction has been confirmed in all our experimental cases except one (Archer, Bourne, & Brown, 1955), where the group with five irrelevant dimensions made more than the expected number of errors. However, the patterns in that study were produced on an oscilloscope and certain apparatus difficulties may have reduced discriminability between the levels of certain dimensions with as many as seven dimensions varying.

Based on our earlier estimates we expect that, if all dimensions have

TABLE 3

COMPUTATION OF THE PROPORTION OF RELEVANT CUES ( $r$ ) IN A FOUR-CHOICE PROBLEM, BASED ON RESULTS OF CORRESPONDING TWO-CHOICE PROBLEMS, WITH VARIABLE NUMBER OF INDEPENDENT IRRELEVANT DIMENSIONS\*

	Number of Irrelevant Dimensions		
	1	2	3
Mean errors, two-choice problem	5.3	8.6	15.0
Predicted $r$ , four-choice problem	.136	.105	.082
Estimated $r$ , four-choice problem	.170	.127	.101
Predicted errors, four-choice problem	27.6	39.3	53.0
Observed mean errors, four-choice problem	21.2	30.5	32.5

\* Walker data.

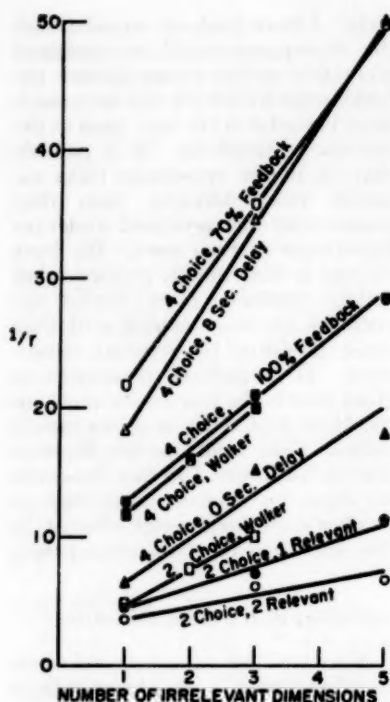


FIG. 3.  $1/r$  as a function of the number of irrelevant stimulus dimensions. Data are abstracted from several of the reported experiments.

equal weight when irrelevant,

$$r = \frac{\frac{1}{2}R}{R + I + B}$$

where  $R$  is the number of relevant dimensions,  $I$  is the number of irrelevant dimensions, and  $B$  is the amount of residual or background irrelevant stimulation from apparatus, surround, and internal cues. Simple algebraic rearrangement of the above equation shows that

$$1/r = \frac{2(R + B)}{R} + \left(\frac{2}{R}\right) I$$

If we assume that in a given experimental situation  $B$  is constant, and if we hold  $R$  fixed, the reciprocal of  $r$  is a linear function of  $I$ . To test the predicted relationship between  $r$  and  $I$ , we have plotted  $1/r$ , estimated from most of the available data, as a function of  $I$  (see Fig. 3). The omitted cases are very much like the ones shown. It will be seen that  $1/r$  in fact does plot as a straight-line function of  $I$  and that the deviations are not systematic.

This is the first test of the functional relationship between number of irrelevant dimensions and the rate of learning. The theory states that learning rate depends on the relative amounts of relevant and irrelevant cues, from which it follows that increasing the number of irrelevant cues should retard learning. It is not surprising that performance gets worse with more irrelevant dimensions—this could occur for any of several reasons. However, since we have found repeated confirmation of the particular functional relationship predicted by the theory, we feel some confidence that the theory correctly states how these irrelevant cues produce their detrimental effect.

Our prediction formula assumes that the measure of irrelevant cues increases proportionally as the number of irrelevant dimensions is increased. This assumption implies additivity of irrelevant cues, and its confirmation supplements previous findings on additivity of relevant cues (Restle: 1955, 1957b, 1958, 1959, and the results reported above).

#### *The Effects of Delaying Reinforcement*

In our experiments we are able to introduce controlled delays between the termination of a stimulus pattern and the onset of the reinforcing light. The experimenter's procedure is like

that in trace conditioning. Our interpretation is that the cues arising from the pattern will decay after the pattern is terminated, so that only a part of them will be present to be conditioned or adapted when reinforcement occurs. The set of cues still present after a certain time interval measured from the termination of the pattern will be called the "trace."

A quantitative theory of the spontaneous change in a set of cues over time has already been suggested by Estes in his stimulus-fluctuation theory (Estes, 1955), which we adapt to our needs. At any time during the delay period, from the end of the presentation of the pattern to the onset of the reinforcement, cues may disappear. We assume that the probability of a cue disappearing (in a given short interval) is independent of how long that cue has already survived. Suppose that the probability of disappearing in a unit interval is  $1-d$ , and the probability of surviving is  $d$ . The probability that a cue survives one such unit interval is  $d$ , the probability that it survives two intervals is  $d^2$ , and that it survives  $t$  intervals is  $d^t$ . This line of reasoning, with suitable adjustments to deal with continuous time, yields an exponential curve of decay.

On different trials different cues will survive the delay interval. Eventually, then, all relevant cues can be conditioned and all irrelevant cues can be adapted. If this occurs, asymptotic performance will be perfect. Accordingly, we expect that delay of reinforcement will not prevent Ss from attaining perfect performance. However, the random decay process during delays has the effect of permitting only a fraction of the original set of cues to be present at reinforcement. Reinforcement

leads to conditioning and adaptation of cues which are present at the time of reinforcement, but has no effect on other cues. Thus the probability that a certain relevant cue will be conditioned on a given trial is reduced, since it will be conditioned only if both of two events occur jointly: (a) the cue survives the delay period and is present, an event which has probability  $d^t$ ; and (b) the cue if present is conditioned, an event with probability  $\theta_0 = \frac{1}{2}r$ . The probability that a cue is conditioned, if the delay is of  $t$  seconds, would then be

$$\theta_t = \theta_0 d^t \quad [15]$$

Exactly the same argument applies to irrelevant cues and the probability that they will be adapted. We assume that both relevant and irrelevant cues decay at the same rate, hence the proportion of relevant cues  $r$  remains constant, but the rate of learning decays exponentially with  $t$ .

Data suitable to test these predictions have been collected (Bourne, 1957). In the experiment the response, pressing a button, terminated the stimulus pattern at once, but the reinforcing signal was delayed by a controlled amount of time, 0, .5, 1, 2, 4, or 8 sec. for different groups. The experiment used self-paced four-choice problems with 1, 3, or 5 irrelevant dimensions, making a total of 18 groups with nine Ss in each group.

All Ss reached criterion of 32 consecutive correct responses, confirming our prediction that problems with delay of reinforcement are solvable.

Using the data of the three groups with 0 delay of reinforcement and 1, 3, and 5 irrelevant dimensions, we estimate three values of  $r$ , having  $\theta_0 = \frac{1}{2}r$ . Our hypothesis states that there is some constant,  $d$ , between zero and one such that  $\theta_t = \theta_0 d^t$ . To test this we estimated each of the 15

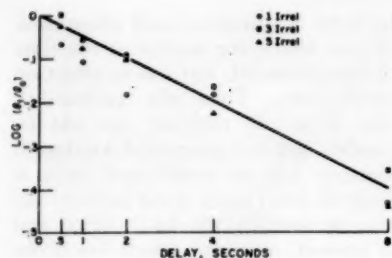


FIG. 4. Log learning rate as a function of delay of information feedback. Solid line is fitted.

values of  $\theta$  from the data of groups which had delays of reinforcement, assuming that  $\theta_0$  of Equation 15 would depend only on the number of irrelevant dimensions. The first question is whether the data would fit Equation 15 with a single parameter  $d$ . A useful transformation is to take the logarithm of both sides of Equation 15, obtaining

$$\log(\theta_t) = \log(\theta_0) + t \log(d) \quad [16]$$

or

$$\log(\theta_t/\theta_0) = t \log(d)$$

Our prediction is now: if we plot our estimated values of  $\theta_t$  in the form  $\theta_t/\theta_0$ , the values should describe a descending straight line from the origin with a slope of  $\log(d)$ .

These estimates were made and the transformed data are shown in Fig. 4. We see that  $\log(\theta_t/\theta_0)$  plots as a descending straight line as required by the simple decay hypothesis. The observed differences among the three sets of points may reasonably be attributed to small chance deviations in the estimates of  $\theta_0$ , which are used as if they were fixed values in these computations.

From the plot in Fig. 4 we can esti-

mate that  $d = .89$ , so that  $\theta_t = \theta_0 (.89)^t$ . Using this as an approximation we compute  $\theta_t$  for each time delay and each level of irrelevant cues. Then, by use of Equation 11, we can reconstruct the complete table of mean errors as a function of delay of reinforcement. If the reconstruction is successful, it suggests that the model correctly describes deterioration of performance as a function of delay of reinforcement and that the decay constant does not depend on the number of irrelevant dimensions. The results of this computation are shown graphically in Fig. 5. The deviations are not systematic, the largest discrepancies being at large mean error scores where the obtained scores are quite variable.

Since our calculations do not lead to serious error, it appears that our hypotheses may be approximately correct. That is, first, the stimulus trace dissipates exponentially during the interval between the end of the pattern presentation and the onset of reinforcement. Second, learning takes place at the time of reinforcement, acting only on those cues which survive the delay; the stimulus variabil-

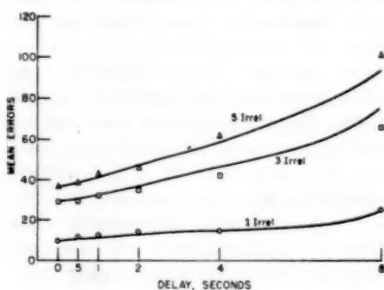


FIG. 5. Mean number of errors as a function of delay of information feedback and number of irrelevant stimulus dimensions. Solid lines are theoretical.

ity induced by the delay retards learning. A previous study had shown that controlled stimulus variability has just such an effect on the rate of learning (Burke, Estes, & Hellyer, 1954). Third, the rate of decay of stimulus traces, about 11% per second, is independent of whether the trace is of relevant or irrelevant dimensions and is not affected by the proportion of relevant cues in the problem.

*The Effects of Omitting Feedback on Some Trials*

Previous studies of two-choice discrimination with simpler experimental procedures have indicated that no learning occurs on trials without reinforcement (e.g., Atkinson, 1956). This result suggested the formulation given earlier that when no reinforcement occurs the values of  $F(k, n)$  and  $A(k', n)$  remain constant. Our attempt to produce this result experimentally led to some interesting complications.

In the study to be discussed (Bourne & Pendleton, 1958), the main variable was the frequency of "blank trials," trials on which no information feedback was given. On a blank trial, the pattern was terminated at the response, but none of the signal lights which indicate the correct answer was lit. After 5 sec. a new pattern was presented, beginning the next trial. The proportion of reinforced trials was set at 1.0 (control), .9, .8, and .7, and the experiment was repeated with problems having 1, 3, or 5 irrelevant dimensions; the arrangement yields a  $3 \times 4$  factorial design with 12 groups of Ss. Self-paced four-choice problems were used throughout.

Since, theoretically, no learning

occurs on blank trials, the result should be a simply deduced reduction in the mean learning rate (the mean computed over training trials). If reinforcement is given only on  $\pi$  of the trials at random, Equation 3 and Equation 6 should apply. We can estimate  $\tau$  from the control groups by use of Fig. 1, and then predict the learning rates in the other groups by theory. The resulting predictions were not accurate. The groups with blank trials learned more slowly than predicted, suggesting that the blank trials interfere with learning on trials with reinforcement.

The findings of the previous study of delayed reinforcement suggest a possible source of interference; on blank trials the pattern trace persists, and when the next trial begins those traces interfere with the cues of the new pattern.

According to our findings on the previous experiment, the decay of a trace may be represented by the function  $d^t$ , where  $d$  is approximately .89. In the present experiment, 5 sec. intervened between the response and the presentation of the next pattern. In this time the trace would decay to a strength of  $(.89)^5 = .56$  of its previous strength. This remainder would interfere with learning on the next trial. Since any mix between two successive patterns is irrelevant (the sequence ensures this, since the same response is never correct twice in a row in any of the four-choice problems), we should expect that, on the trial just following a blank trial, .56 of the relevant cues would be made irrelevant.

This means that there are three kinds of trials with three different learning rates; (a) trials with no reinforcement, on which  $\theta = 0$ ; (b) trials following nonreinforced trials, on

which  $\theta = (1 - .56)(r/2)$ ; and (c) other trials, on which  $\theta = r/2$ .

In the sequence, two blank trials never followed one another. Thus, when reinforcements were given on  $\pi$  of the trials, there were  $1 - \pi$  blank trials, and  $1 - \pi$  trials following blank trials. The mean learning rate is therefore

$$\begin{aligned}\bar{\theta}_r &= (1 - \pi)(0) \\ &\quad + (1 - \pi)(1 - .56)(r/2) \\ &\quad + (1 - (2 - 2\pi))(r/2) \\ &= (1.56\pi - .56)(r/2)\end{aligned}$$

We can estimate  $r$  from the data of the control group with no blank trials, i.e., with  $\pi = 1$ . Then we can compute  $\bar{\theta}_r$ , since we know  $r$ , and  $\pi$  is an experimentally controlled probability. Knowing  $\bar{\theta}$  and  $r$ , we can use Equation 3 to predict the total errors expected in solving the problem. Results of these computations are shown in Table 4. The results of the theoretical computations are

shown by the solid lines in Fig. 6. Only one control group ( $\pi = 1$ ,  $I = 1$ ) was used for purposes of estimating the values of  $r$ ; all other values were computed solely by reference to the experimental procedure and through the use of information gleaned from the previous experiment about the rate of decay of pattern traces.

The reader is cautioned that we have not established that stimulus traces from nonreinforced trials interfere and retard learning. We have, however, shown that if this is the case the result should be exactly what was observed. A further study is needed in which blank trials are introduced but in which the time interval between the end of the pattern presentation and the initiation of the next trial is varied. In the present study that interval was fixed at 5 sec. for all groups. If it had been, say, 20 sec., we should expect that  $\theta_r$  would almost equal  $\pi\theta_1$ , and our first computation would have been successful. If the interval were less than 5 sec.,

TABLE 4  
THE EFFECT OF BLANK TRIALS ON TOTAL ERROR SCORES

Number of Irrel. Dim.	Proportion of Feedback Trials			
	1.0	.9	.8	.7
1	$r$ (Estimated)	.157	.157	.157
	$\theta_1$ (Predicted)	.0785	.0666	.0557
	Errors (Pred.)	(—) <sup>a</sup>	26.7	31.9
	Errors (Obt.)	22.6	26.8	31.0
3	$r$ (Predicted)	.0964	.0964	.0964
	$\theta_1$ (Predicted)	.0482	.0409	.0342
	Errors (Pred.)	43.5	51.3	61.4
	Errors (Obt.)	42.6	52.0	67.9
5	$r$ (Predicted)	.0695	.0695	.0695
	$\theta_1$ (Predicted)	.0347	.0295	.0246
	Errors (Pred.)	67.3	79.4	95.3
	Errors (Obt.)	66.9	80.0	97.7

<sup>a</sup> Data from this group were used in calibration.



however, the discrepancy should be even larger.

### DISCUSSION

In this paper we have reported an attempt to extend a theory of discriminative learning so as to analyze the process of identifying concepts. We have drawn on two sources: (a) theoretical analyses of animal and human learning in simple discrimination problems (Atkinson, 1956; Estes, 1955; Restle: 1955, 1957a, 1958) and (b) the procedures of concept-identification experiments (Bourne, 1957; Bourne & Haygood, 1959; Bourne & Pendleton, 1958; Walker, 1958).

The analysis arises from the assumption that concept identification is a slight complication of discrimination learning. We have employed a theory which states that discrimination learning involves two processes—conditioning relevant cues and adapting irrelevant cues. The rate of these processes depends on the proportion of relevant cues and the probability that a cue is present at the time of reinforcement. In the experiments various stimulus dimensions such as color, size, shape, number, and position were varied in some problems and not in others. When varied, a dimension could be relevant (correlated with reinforcements) or irrelevant. It is natural to attach hypothetical sets of cues to each such dimension, calling the cues relevant or irrelevant according to how the dimension is reinforced. In one experiment reinforcement was delayed after the termination of the stimulus presentation. Our theoretical interpretation was that the trace of the stimulus would decay during that period (i.e., that cues would drop out randomly). A simple form of such

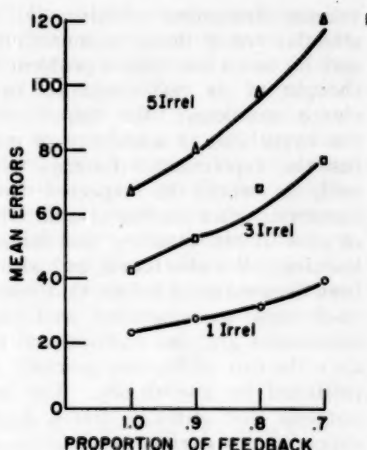


FIG. 6. Mean number of errors as a function of proportion of reinforced trials and number of irrelevant stimulus dimensions. Solid lines are theoretical.

decay suggested by Estes' notion of fluctuation of stimulus elements leads to predictions that delay of reinforcement would not prevent perfect performance but would retard learning, the rate of learning being an exponential decay function of the duration of delay. In another experiment, some trials were left unreinforced. To analyze the results we drew on Atkinson's (1956) suggestion that no learning occurs on an unreinforced trial, and also used the fact that stimulus traces would be present to confuse *S*. In each case we called on our psychological intuition to decide how the variables would probably enter into performance. The theory of discrimination learning was then used to put these ideas into mathematical form and to mediate quantitative predictions.

The quantitative predictions may be classified by asking three main questions: (a) are the cues from

various dimensions additive, (b) do stimulus traces decay exponentially, and (c) can a four-choice problem be thought of as two conjoined two-choice problems? We found that the hypothesis of additivity of cues fits the experimental findings very well, as would be expected from numerous other studies of additivity of cues in rat, monkey, and human learning. We also found, as had not been demonstrated before, that irrelevant cues from several irrelevant dimensions are also additive and reduce the rate of learning precisely as predicted by the theory. The hypothesis that stimulus traces decay exponentially during the interval of delay of reinforcement and that such decay reduces the rate of learning was supported by experimental data. Furthermore, we found evidence that such traces can have an interfering effect on immediately succeeding patterns in the event that reinforcements are omitted on some trials. Again, a quantitative hypothesis fit the data quite well. In regard to the hypothesis that our four-choice problems can be thought of as conjunctions of two-choice problems, we found our only conflict of evidence. The success of the theory in handling four-choice problems, as in the experiments on additivity of cues and delay or omission of reinforcement, suggests that the hypothesis is admissible. In two studies we tried to predict performance on a four-choice problem from data on corresponding two-choice problems. In one study the predictions were close, in the other they were clearly erroneous. Since we are not at all sure of the cause of this discrepancy, we must await further results.

While we feel that the logical and mathematical steps of our develop-

ment and the procedures of the experiments are defensible, there is a looseness in the tests of theoretical points which we have not been able to prevent. Our procedure has been to estimate parameters (in this theory the measures of certain sets of cues) from part of the data of an experiment, and then use the theory to predict statistics of the rest of the data (total errors to criterion of other groups.) One naturally wants to know how well these predictions accord with observation, and we have shown the results graphically. One also wants to know whether the observed discrepancies are sufficient to call for revision or rejection of the theory. On this point we are unable to offer clear answers. Formally our problem is a statistical one of testing the theoretical hypothesis (playing the role of a null hypothesis) against all alternatives. The theoretical predictions depend on estimates of parameters and are themselves subject to sampling variations. Unfortunately our methods of estimation do not include ways of evaluating the stability of the estimates, so that we do not know in full detail what we are predicting—we cannot prove that the estimates are unbiased, and we do not know their sampling distribution. This is a situation common in mathematical learning theory at present. These difficulties prevent accurate statistical evaluation of the theoretical predictions. If we ask whether the data differ significantly from the point predictions made above, we should reject a true theory too often, for the only prediction the theory can make is subject to random deviations. The test would be invalid. Furthermore, to obtain specific predictions we have inserted several special assumptions, particularly the assumption

that all dimensions have the same weight. This is certainly not exactly true, but we have insufficient data to judge just how wrong it is. If we had such data we should dispense with the assumption, but that would require much larger experiments, beyond the limits of feasibility. Rather than report statistical tests which are not appropriate to the situation, we have preferred to present our results in graphs and tables and let the reader decide, as we have had to decide, to what degree the predictions are accurate enough to support the theory and in what ways the theory should be improved. We bow to convention just enough to remark that the predictions we have described as "accurate," all except the one involving Walker's data, would be acceptable even using the overly stringent *t* test with the point prediction taken as a fixed parameter.

It should be evident that we have not offered a comprehensive description of all the important factors in concept-identification problems. We have dealt only with the probability of a correct response and have not gone into the "strategies" by which success is attained (Bruner, Goodnow, & Austin, 1956). We have left aside differences between Ss in intellect and personality, and we have explored only a modest variety of stimuli and reinforcement schedules. Within the restricted scope of the investigation we have striven for, and to some degree attained, precision in theoretical formulation and accuracy in detailed quantitative predictions of large amounts of data. It is our hope that such results will facilitate investigations of the variables we have ignored, and that our findings will be helpful in formulating more general theories with strong roots in experimental data.

To us, these results demonstrate the value of parametric experiments which explore many values of several important variables at once, and indicate that such experimental results may refine and test proposed mathematical models. We hope that the orderly and predictable results found here will encourage precise theoretical and experimental work in human cognition, where dependable scientific understanding can have broad practical as well as theoretical significance.

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## MOTIVATION RECONSIDERED: THE CONCEPT OF COMPETENCE

ROBERT W. WHITE

*Harvard University*

When parallel trends can be observed in realms as far apart as animal behavior and psychoanalytic ego psychology, there is reason to suppose that we are witnessing a significant evolution of ideas. In these two realms, as in psychology as a whole, there is evidence of deepening discontent with theories of motivation based upon drives. Despite great differences in the language and concepts used to express this discontent, the theme is everywhere the same: Something important is left out when we make drives the operating forces in animal and human behavior.

The chief theories against which the discontent is directed are those of Hull and of Freud. In their respective realms, drive-reduction theory and psychoanalytic instinct theory, which are basically very much alike, have acquired a considerable air of orthodoxy. Both views have an appealing simplicity, and both have been argued long enough so that their main outlines are generally known. In decided contrast is the position of those who are not satisfied with drives and instincts. They are numerous, and they have developed many pointed criticisms, but what they have to say has not thus far lent itself to a clear and inclusive conceptualization. Apparently there is an enduring difficulty in making these contributions fall into shape.

In this paper I shall attempt a conceptualization which gathers up some of the important things left out by drive theory. To give the concept a name I have chosen the word *competence*, which is intended in a broad bio-

logical sense rather than in its narrow everyday meaning. As used here, competence will refer to an organism's capacity to interact effectively with its environment. In organisms capable of but little learning, this capacity might be considered an innate attribute, but in the mammals and especially man, with their highly plastic nervous systems, fitness to interact with the environment is slowly attained through prolonged feats of learning. In view of the directedness and persistence of the behavior that leads to these feats of learning, I consider it necessary to treat competence as having a motivational aspect, and my central argument will be that the motivation needed to attain competence cannot be wholly derived from sources of energy currently conceptualized as drives or instincts. We need a different kind of motivational idea to account fully for the fact that man and the higher mammals develop a competence in dealing with the environment which they certainly do not have at birth and certainly do not arrive at simply through maturation. Such an idea, I believe, is essential for any biologically sound view of human nature.

As a first step, I shall briefly examine the relevant trends of thought in several areas of psychology. From this it will become clear that the ideas advanced in this paper have already been stated, in one way or another, by workers in animal behavior, child development, cognitive psychology, psychoanalytic ego psychology, and the psychology of personality. If there is novelty in this essay, it lies in putting together

pieces which are not in themselves new. They already lie before us on the table, and perhaps by looking once more we can see how to fit them into a larger conceptual picture.

#### THE TREND IN ANIMAL PSYCHOLOGY

One of the most obvious features of animal behavior is the tendency to explore the environment. Cats are reputedly killed by curiosity, dogs characteristically make a thorough search of their surroundings, and monkeys and chimpanzees have always impressed observers as being ceaseless investigators. Even Pavlov, whose theory of behavior was one of Spartan simplicity, could not do without an investigatory or orientating reflex. Early workers with the obstruction method, such as Dashiell (1925) and Nissen (1930), reported that rats would cross an electrified grid simply for the privilege of exploring new territory. Some theorists reasoned that activity of this kind was always in the service of hunger, thirst, sex, or some other organic need, but this view was at least shaken by the latent learning experiments, which showed that animals learned about their surroundings even when their major needs had been purposely sated. Shortly before 1950 there was a wave of renewed interest not only in exploratory behavior but also in the possibility that activity and manipulation might have to be assigned the status of independent motives.

#### *Exploratory Behavior*

In 1953 Butler reported an experiment in which monkeys learned a discrimination problem when the only reward was the opening of a window which permitted them to look out upon the normal comings and goings of the entrance room to the laboratory. The discriminations thus formed proved to be resistant to extinction. In a later

study, Butler and Harlow (1957) showed that monkeys could build up a series of four different discriminations solely for the sake of inspecting the entrance room. Butler concluded that "monkeys—and presumably all primates—have a strong motive toward visual exploration of their environment and that learning may be established on the basis of this motive just as it may be established on the basis of any motive that regularly and reliably elicits responses." Montgomery, in 1954, reported a study with rats in which the animals, their major organic needs satiated, learned to avoid the short arm of a Y maze and to take the path which led them into additional maze territory suitable for exploration. Similar findings have been described by Myers and Miller (1954), whose rats learned to press a bar for the sake of poking their heads into a new compartment and sniffing around. Zimbardo and Miller (1958) enlarged upon this study by varying the amount of novelty in the two compartments. In their report "the hypothesis advanced is that opportunity to explore a 'novel' environment or to effect a stimulus change in the environment is the reinforcing agent."

These experiments make a strong case for an independent exploratory motive. The nature of this motive can be more fully discerned in situations in which the animals are allowed a varied repertory of behavior. In 1950 Berlyne published a searching paper on curiosity, a theme which he further developed in subsequent years (1955, 1957, 1958). The rats in his experiments were confronted with an unfamiliar space and later with various novel objects placed in it. Approaching, sniffing, and examining were readily elicited by each novelty, were fairly rapidly extinguished, but were restored nearly to original strength



when a fresh novelty was added. Exploration on the part of chimpanzees has been studied by Welker (1956), who put various pairs of objects before the animals and observed the course of their interest. The objects were often first approached in a gingerly manner, with signs of uneasiness, then examined and handled quite fully, then discarded. Introducing a new pair of objects promptly reproduced the whole sequence, just as it did with the rats in Berlyne's experiments. Welker used pairs of objects to find out whether or not the chimpanzees would have common preferences. Bigness and brightness evoked more interest, and greater time was spent upon objects which could be moved, changed, or made to emit sounds and light.

Recent reviews by Butler (1958) and Cofer (1959) show that a great deal of similar work is going on in animal laboratories, generally with similar results.

#### *Exploration as a Drive*

The designers of these experiments have favored the idea that exploration should be listed as an independent primary drive. In all cases the experimental plan calls for the elimination of other primary drives by satiation. It is recognized, however, that a confirmed advocate of orthodoxy might bring up two objections to the proposed enlargement of the list of primary drives. He might claim that exploratory behavior could be explained as a consequence of secondary reinforcement, or he might contend that it is reinforced by reduction of anxiety.

The first argument meets an immediate difficulty in Butler's finding that discriminations learned on the basis of visual exploration are resistant to extinction. When reinforcement of primary drive never takes place in the experimental situation, it is to be ex-

pected that secondary reinforcement will not prevent extinction (Miller, 1951). But even in those cases where extinction is rapid, as it was with Berlyne's rats and Welker's chimpanzees, serious problems are raised by the quick recovery of exploratory behavior when a novel stimulus is introduced (Berlyne, 1950). In order to sustain the idea that secondary reinforcement accounts for this fact, we should have to suppose that primary rewards have often been connected with the exploration of novelties. It would have to be assumed, for instance, that the securing of food by young animals occurred with considerable frequency in connection with the investigation of novel objects. This image may seem to fit mature animals who search the environment for their food, but it certainly cannot apply to young mammals before they are weaned. Here the learning process can do virtually nothing to reinforce an interest in novelties. Gratification comes from following the same old cues to the same old consummatory responses, and the animal whose attention strays to some novel variation of the breast will only find himself frustrated. One can say that the whole mammalian pattern of infancy works in the opposite direction. The mother is more active than the young in providing gratifications, and the babies must be pursued and retrieved if they stray from the scene of her ministry. However one looks at it, the hypothesis of secondary reinforcement seems to me to demand improbable assumptions about the relationship in the lives of young animals between exploration and primary need gratification.

The hypothesis that exploratory behavior is related to fear and receives its reinforcement from the reduction of anxiety is at first glance considerably more plausible. It seems justified by the observation that Welker's chimpan-

zees showed uneasiness on first contact with novel objects, and it fits the behavior of rats in a new maze, as reported by Whiting and Mowrer (1943), where initial terror gave place to an exploration so feverish that the food reward was not eaten. Montgomery and Monkman (1955) have undertaken to challenge this hypothesis by a direct experimental attack. They showed that fear induced in rats before entering a novel situation did not increase exploratory behavior, and that fear induced within the novel situation decreased exploration to an extent correlated with the intensity of the fear. They find it more reasonable to suppose that fear and exploration are conflicting forms of behavior, and this view can also be defended on purely logical grounds. Fear shows itself in either freezing or avoidance, whereas exploration is clearly an instance of approach. There is hardly a more perfect example of conflict between incompatible responses than that of an animal hesitating between investigation and flight. It is clear that exploration can sometimes serve to reduce anxiety, but the proposition that it comes into existence only for this purpose cannot be so easily accepted.

What assumptions have to be made to support the thesis that exploration is motivated by anxiety reduction? It has to be assumed that certain characteristic stimuli arouse anxiety and that exploration of these stimuli is then found to reduce the anxiety. If the characteristics in question are those of novelty and unfamiliarity, we must heed Berlyne's reminder that for the infant all experience is novel and unfamiliar. Berlyne (1950) proposes that the exploratory reaction "may be one that *all* stimuli originally evoke, but which disappears (becomes habituated) as the organism becomes familiar with them." But if all stimuli at first

arouse anxious tension, we would have to deduce that all response would consist of avoidance in the interest of reducing that tension. Approaching a stimulus and taking steps to increase its impact could not occur. An exploratory tendency must be there in the first place before it can achieve the function of reducing anxiety. As Woodworth (1938) expresses it, "if there were no exploratory drive to balance and overbalance the fear drive, an animal would be helpless in a novel situation." I find it hard to believe that creatures so liberally endowed with fear could ever achieve a working mastery of the environment if they were impelled toward it only by the pressure of organic needs.

Both hypotheses thus far examined—secondary reinforcement and anxiety reduction—require us to make improbable assumptions. There remains the possibility that exploration should simply be added to the list of primary drives and otherwise treated in orthodox fashion. Myers and Miller (1954) suggest that this is the appropriate course, provided the new drive shows the same functional properties as those already known. "If an exploratory tendency can produce learning like other drives such as hunger, and also show a similar pattern of satiation and recovery, these functional parallels to already known drives would help to justify its classification in the same category." Logically the problem can be dealt with in this way, but we must consider very carefully what happens to the category of drive if we admit this new applicant to membership.

Using hunger as the chief model, the orthodox conception of drive involves the following characteristics: (a) there is a tissue need or deficit external to the nervous system which acts upon that system as a strong persisting stimulus; (b) this promotes activity

which is terminated by a consummatory response with consequent reduction of need; (c) the reduction of need brings about the learning which gradually shapes behavior into an economical pursuit of suitable goal objects. In this scheme the tension of an aroused drive is interpreted as unpleasant, at least in the sense that the animal acts in such a way as to lower the drive and becomes quiescent when it is lowered. There are probably no living champions of so simple an orthodoxy, yet the scheme remains pervasive, and it is therefore worth while to observe that the proposed exploratory drive hardly fits it at all.

In the first place, the exploratory drive appears to bear no relation whatever to a tissue need or deficit external to the nervous system. It is, of course, clearly related to certain characteristics of stimulation from the external environment, a source of motivation which Harlow (1953) would like to see restored to a serious place in contemporary psychology; but it certainly cannot be correlated with a visceral need comparable to hunger, thirst, or sex. Considering the pattern of satiation and recovery shown by Welker's chimpanzees, Woodworth (1958) remarks that "what becomes satiated is not the exploratory tendency in general, but the exploring of a particular place or object." It is possible, as Hebb (1955) has pointed out, that the so-called "reticular activation system" in the brain stem creates a kind of general drive state, and this mechanism might indeed be flexibly responsive to changes in sensory stimulation. This interesting suggestion, however, is still a far cry from viscerogenic drives; it commits us instead to the novel idea of a neurogenic motive, one in which the state of the nervous system and the patterns of external stimulation conspire to produce motivated behavior. There is even

a good deal of trouble in supposing that the adequate stimuli for exploration are either strong or persistent. Novelty certainly cannot be equated with strength or persistence, and animals seem readily able to disregard the stimuli to exploration when they are weary.

In the second place, exploratory behavior cannot be regarded as leading to any kind of consummatory response. It is usual for the animal's investigation to subside gradually. If the animal at some point turns away and leaves the once novel object we may say that its curiosity is "satisfied," but we do not mean by this that the equivalent of a consummatory response has just taken place. The sequence suggests rather that curiosity wears out and slowly falls to a level where it no longer guides behavior, at least until a fresh novelty comes into view.

Finally, in the case of exploratory behavior there is real difficulty in identifying reinforcement with need reduction. Montgomery (1954), describing the learning of the Y maze, points out that the short arm, essentially a dead end, would tend to reduce the exploratory drive, whereas the long arm, itself a complex maze, would increase it—but the long arm is chosen. If the long arm functions as a reinforcing agent, "the mechanism underlying this reinforcement is an *increase*, rather than a decrease, in the strength of the exploratory drive." In this experiment, as in their natural habitat, animals do not wait to have novelty thrust upon them, nor do they avoid situations in which novelty may be found. Such behavior can be most readily conceptualized by admitting that under certain circumstances reinforcement can be correlated with an increase in arousal or excitement rather than a decrease. A drive which has no consummatory climax seems almost to require this formula-

tion. It is distinctly implausible to connect reinforcement with the waning of an agreeable interest in the environment or with a general progress from zestful alertness to boredom.

If we admit exploration to the category of drive we are thus committing ourselves to believe that drives need have no extraneural sources in tissue deficits or visceral tensions, that they are not necessarily activated by strong or persistent stimuli, that they do not require consummatory responses, and that drive increase can sometimes be a mechanism of reinforcement.

#### *Activity and Manipulation*

Exploration is not the only motive proposed by critics of drive orthodoxy, and novelty is not the only characteristic of the environment which appears to incite motivated behavior. Some workers have suggested a need for activity, which can be strengthened by depriving animals of their normal opportunities for movement. Kagan and Berkun (1954) used running in an activity wheel as the reward for learning and found it "an adequate reinforcement for the instrumental response of bar pressing." Hill (1956) showed that rats will run in an activity wheel to an extent that is correlated with their previous degree of confinement. It is certain that the activity wheel offers no novelty to the animals in these experiments. Nevertheless, they seem to want to run, and they continue to run for such long times that no part of the behavior can readily be singled out as a consummatory response. Perhaps an unpleasant internal state created by inactivity is gradually worked off, but this is certainly accomplished by a tremendous increase of kinaesthetic stimulation and muscular output which would seem to imply increased excitation in the system as a whole.

Harlow and his associates (Harlow, 1953; Harlow, Harlow, & Meyer, 1950) maintain that there is also a manipulative drive. It is aroused by certain patterns of external stimulation and reduced by actively changing the external pattern. The experiments were done with rhesus monkeys, and they involve the solving of a mechanical problem which, however, leads to no further consequences or rewards. The task might be, for instance, to raise a hasp which is kept in place by both a hook and a pin; all that can be accomplished is to raise the hasp, which opens nothing and leads to no fresh discoveries. When the hasp problem is simply installed in the living cages, the monkeys return to it and solve it as many as 7 or 8 times over several days. It seems unlikely that novelty can be postulated as the essential characteristic of the stimulus which evokes this repeated behavior. The simplest interpretation is rather that value lies for the animal in the opportunity, as Zimbardo and Miller (1958) express it, "to effect a stimulus change in the environment." This formulation suggests something like the propensities toward mastery or power that have often been mentioned in discussions of human motivation.

The addition of activity and manipulation to the list of primary drives can only make more serious the difficulties for the orthodox model that resulted from admitting exploration. But recent research with animals has put the orthodox model on the defensive even on its home grounds. It has become increasingly clear that hunger, thirst, and sex cannot be made to fit the simple pattern that seemed so helpful 40 years ago.

#### *Changing Conceptions of Drive*

In a brief historical statement, Morgan (1957) has pointed out that the

conception of drive as a noxious stimulus began to lose its popularity among research workers shortly after 1940. "On the whole," he says, "the stimulus concept of drive owed more to wishful thinking than to experimental fact." When technical advances in biochemistry and brain physiology made it possible to bring in an array of new facts, there was a rapid shift toward the view that "drives arise largely through the internal environment acting on the central nervous system." One of the most influential discoveries was that animals have as many as a dozen specific hungers for particular kinds of food, instead of the single hunger demanded by Cannon's model of the hunger drive. If an animal's diet becomes deficient in some important element such as salt, sugar, or the vitamin-B complex, foods containing the missing element will be eagerly sought while other foods are passed by, a selectivity that obviously cannot be laid to contractions of the stomach. Similarly, a negative food preference can be produced by loading either the stomach or the blood stream with some single element of the normal diet. The early work of Beach (1942) on sexual behavior brought out similar complications in what had for a time been taken as a relatively simple drive. Hormone levels appeared to be considerably more important than peripheral stimulation in the arousal and maintenance of the sex drive. Further work led Beach (1951) to conclude that sexual behavior is "governed by a complex combination of processes." He points out that the patterns of control differ tremendously from one species to another and that within a single species the mechanisms may be quite different for males and females. Like hunger, the sex drive turns out to be no simple thing.

New methods of destroying and of

stimulating brain centers in animals have had an equally disastrous effect on the orthodox drive model. The nervous system, and especially the hypothalamus, appears to be deeply implicated in the motivational process. Experimental findings on hypothalamic lesions in animals encourage Stellar (1954) to believe that there are different centers "responsible for the control of different kinds of basic motivation," and that in each case "there is one main excitatory center and one inhibitory center which operates to depress the activity of the excitatory center." As research findings accumulate, this picture may seem to be too cleanly drawn. Concerning sexual behavior, for example, Rosvold (1959) concludes a recent review by rejecting the idea of a single center in the cerebrum; rather, the sex drive "probably has a wide neural representation with a complex interaction between old and new brain structures and between neural and humoral agents." Nevertheless, Miller's (1958) careful work seems to leave little doubt that motivated behavior in every way similar to normal hunger and normal pain-fear can be elicited by electrical stimulation of quite restricted areas of the hypothalamus. It is clear that we cannot regress to a model of drives that represents the energy as coming from outside the nervous system. Whatever the effects of peripheral stimulation may be, drives also involve neural centers and neural patterns as well as internal biochemical conditions.

What sort of model becomes necessary to entertain these newly discovered facts? In 1938 Lashley expressed the view that motivation should not be equated with disturbance of organic equilibrium but rather with "a partial excitation of a very specific sensorimotor mechanism irradiating to affect other systems of reaction." Beach



(1942) postulated that there must be in the nervous system "a condition analogous to Sherrington's central excitatory state." Morgan, in 1943, undertook to capture the facts in a systematic theory which seems to have been well sustained by subsequent research (Morgan, 1957). He distinguished two types of process which he called *humoral motive factors* and *central motive states*. The humoral factors consist of chemical or hormonal constituents of the blood and lymph, and they are conceived to influence behavior chiefly by a direct sensitizing action on neural centers. The central motive states have several properties: They are partly self-maintaining through neural circuits, they tend to increase the organism's general activity, they evoke specific forms of behavior not strongly controlled by the environment, and they prime or prepare consummatory responses which will occur when adequate stimulation is found. This is a far cry from the orthodox model, but we must nowadays admit that the orthodox model is a far cry from the facts.

In view of this radical evolution of the concept of drive, it is not surprising to find the drive reduction hypothesis in serious difficulties. The earlier identification of reinforcement with drive reduction has been directly attacked in a series of experiments designed to show that learning takes place when drive reduction is ruled out.

In 1950 Sheffield and Roby showed that instrumental learning would take place in hungry rats when the reward consisted not of a nutritive substance but of sweet-tasting saccharine in the drinking water. This finding appeared to be "at variance with the molar principle of reinforcement used by Hull, which identifies primary reinforcement with 'need reduction.'" The authors

naturally do not question the vital importance of need reduction, but they point out that need-reducing events may accomplish reinforcement through a mechanism more direct and speedy than the reduction of the need itself. They think that "stimulation and performance of a consummatory response appears to be more important to instrumental learning—in a primary, not acquired, way—than the drive satisfaction which the response normally achieves." Their findings are in line with an earlier experiment with chickens by Wolfe and Kaplon (1941), who used different sizes of food pellets so that the number of pecks and the amount of food received could be thrown out of their usual close connection. The chickens, we might say, would rather peck than eat; learning was more strongly reinforced when four pecks were necessary than when one peck was enough to take the same amount of food.

The substitution of the consummatory response for need reduction as the immediate reinforcing mechanism is a step in advance, but it soon turns out that another step is required. Can it be shown that an aroused need which does not reach consummation has a reinforcing effect? To test this possibility Sheffield, Wulff, and Backer (1951) provided male rats with the reward of copulating with a female, but not enough times to produce ejaculation. This reward was favorable to instrumental learning even though there was no need reduction and no performance of the final consummatory act. The results were supported by Kagan (1955), whose animals showed substantial learning under the same conditions, though learning was still faster when ejaculation was permitted. Sheffield, Roby, and Campbell (1954) have proposed a *drive-induction* theory according to which the property of re-



inforcement is assigned to the excitement of an aroused drive. We have already seen that some such assumption is essential if exploration is to be assigned the status of a drive. Here it can be added that the whole theory of pregenital sexuality involves motivation without consummatory acts and without any but the most gradual need reduction. And as a final blow to the orthodox hypothesis comes the finding by Olds and Milner (1954) that positive reinforcement can be brought about by direct electrical stimulation of certain areas of the brain. Once again we learn that neural centers are deeply implicated in the plot of motivation. The simple mechanics of need reduction cannot possibly serve as the basis for a theory of learning.

Twenty years of research have thus pretty much destroyed the orthodox drive model. It is no longer appropriate to consider that drives originate solely in tissue deficits external to the nervous system, that consummatory acts are a universal feature and goal of motivated behavior, or that the alleviation of tissue deficits is the necessary condition for instrumental learning. Instead we have a complex picture in which humoral factors and neural centers occupy a prominent position; in which, moreover, the concept of neurogenic motives without consummatory ends appears to be entirely legitimate. Do these changes remove the obstacles to placing exploration, activity, and manipulation in the category of drives?

Perhaps this is no more than a question of words, but I should prefer at this point to call it a problem in conceptual strategy. I shall propose that these three new "drives" have much in common and that it is useful to bring them under the single heading of competence. Even with the loosening and broadening of the concept of drive,

they are still in important respects different from hunger, thirst, and sex. In hunger and thirst, tissue deficits, humoral factors, and consummatory responses retain an important position. The mature sex drive depends heavily on hormonal levels and is sharply oriented toward consummation. Tendencies like exploration do not share these characteristics, whatever else they have in common with the better known drives. It is in order to emphasize their intrinsic peculiarities, to get them considered in their own right without a cloud of surplus meanings, that I prefer in this essay to speak of the urge that makes for competence simply as motivation rather than as drive.

#### THE TREND IN PSYCHOANALYTIC EGO PSYCHOLOGY

Rather an abrupt change of climate may be experienced as we turn from the animal laboratory to the psychoanalytic treatment room, but the trends of thought in the two realms turn out to be remarkably alike. Here the orthodox view of motivation is to be found in Freud's theory of the instincts—they might be known to us as drives if an early translator had been more literal with the German *Trieb*.

#### *Freud's Theories of Instinct and Ego*

In his final work, Freud (1949) described instincts as "somatic demands upon mental life" and as "the ultimate cause of all activity." He wrote further:

It is possible to distinguish an indeterminate number of instincts and in common practice this is in fact done. For us, however, the important question arises whether we may not be able to derive all of these instincts from a few fundamental ones. . . . After long doubts and vacillations we have decided to assume the existence of only two basic instincts, *Eros* and the *destructive instinct* (Freud, 1949, p. 20).

The history of Freud's long doubts and vacillations has been lucidly related by Bibring (1941). Up to 1914 Freud used a two-fold classification of sexual instincts and ego instincts. The ego instincts made their appearance in his case histories in a somewhat moral character, being held responsible for the disastrous repression of sexual needs, but in systematic usage they were conceived as serving the goal of self-preservation, and hunger was generally taken as an appropriate model. In 1914, when he evolved the concept of narcissism and saw that it threatened to blur the line between sexual and ego tendencies, Freud (1925b) still expressed himself as unwilling to abandon an idea which followed the popular distinction of love and hunger and which reflected man's dual existence "as reproducer and as one who serves his own ends." Various facts, particularly those of sadism and masochism, served to overcome his reluctance, so that he finally united self-preservation and preservation of the species under the heading of Eros or life instincts, establishing destructiveness or the death instinct as the great antagonist in a profound biological sense (Freud, 1948). This highly speculative step proved to be too much for some of his otherwise loyal followers, and the earlier orthodoxy did not become entirely extinct.

It is easier to follow Freud's reasoning when we bear in mind the simultaneous development of his ideas about the mental apparatus. Bibring (1941) points out that even in his early thinking a sharp contrast was always drawn between instinct and mental apparatus. Instinct supplied the energy in the form of powerful, persisting internal stimuli; the apparatus guided it into channels which produced organized behavior and eventually put a stop to the

persisting stimulation. In 1915 Freud wrote:

The nervous system is an apparatus having the function of abolishing stimuli which reach it or of reducing excitation to the lowest possible level; an apparatus which would even, if this were feasible, maintain itself in an altogether unstimulated condition. . . . The task of the nervous system is—broadly speaking—to master stimuli (Freud, 1925c, p. 63).

During the next decade there was a considerable growth in his ideas about the mental apparatus, culminating in the well known division into id, ego, and superego. The activities of the ego now received much fuller recognition. Freud (1927) assigned to it "the task of self-preservation," which it accomplished through its several capacities of perception, memory, flight, defense, and adaptive action. One can see Freud's thought moving from a mechanical analogy—an engine and its fuel—toward a much more adaptational conception of the mental apparatus. Ego instincts did not wholly disappear, but the decline in their systematic importance was compensated by the insight that self-perservative tendencies were to some extent built into the whole living system. It is significant that as he took this course he came to question the earlier tension-reduction theory. In the last year of his life he declared it to be probable "that what is felt as pleasure or unpleasure is not the *absolute* degree of the tensions but something in the rhythm of their changes" (Freud, 1949).

Freud's tendency to revise his thinking makes it difficult to pin down an orthodox doctrine, but most workers will probably agree that his main emphasis was upon somatically based drives, a mental apparatus which received its power from the drives, and, of course, the multitude of ways in

which the apparatus controlled, disguised, and transformed these energies. His treatment of the ego was far from complete, and it was not long before voices were raised against the conception that so vital and versatile a part of the personality could be developed solely by libidinal and aggressive energies.

#### *An Instinct to Master*

In 1942 Hendrick proposed that this difficulty be met by assuming the existence of an additional major instinct. "The development of ability to master a segment of the environment," he wrote, and the need to exercise such functions, can be conceptualized as an "instinct to master," further characterized as "an inborn drive to do and to learn how to do." The aim of this instinct is "pleasure in exercising a function successfully, regardless of its sensual value." The simpler manifestations are learning to suck, to manipulate, to walk, to speak, to comprehend and to reason; these functions and others eventually become integrated as the ego. "The central nervous system is more than a utility," Hendrick declared. The infant shows an immediate desire to use and perfect each function as it ripens, and the adult secures gratification from an executive function efficiently performed regardless of its service to other instincts.

Hendrick's procedure in this and two supporting papers (1943a, 1943b) is quite similar to that of the animal psychologists who propose listing exploration as an additional primary drive. The instinct to master has an aim—to exercise and develop the ego functions—and it follows hedonic principles by yielding "primary pleasure" when efficient action "enables the individual to control and alter his environment." It is to this extent analogous to the instincts assumed by Freud. But

just as an exploratory drive seemed radically to alter the whole conception of drive, so the instinct to master implied a drastic change in the psychoanalytic idea of instinct. Critics were quick to point out that Freud had always conceived of instincts as having somatic sources external to the ego apparatus, a condition not met by the proposed instinct to master. There was nothing comparable to erogenous zones, to orgasm, or to the sequence of painful tension followed by pleasurable release. Mastery, the critics agreed, could not be an instinct, whatever else it might be.

It is of interest that Fenichel (1945), who definitely rejected Hendrick's proposal, gives us another close parallel to the animal work by attributing mastering behavior to anxiety-reduction. He argued that mastery is "a general aim of every organism but not of a specific instinct." He agreed that there is "a pleasure of enjoying one's abilities," but he related this pleasure to cessation of the anxiety connected with not being able to do things. "Functional pleasure," he wrote, "is pleasure in the fact that the exercise of a function is now possible without anxiety," and he contended that when anxiety is no longer present, when there is full confidence that a given situation can be met, then action is no longer accompanied by functional pleasure. We must certainly agree with Fenichel that anxiety *can* play the part he assigns it, but the proposal that all pleasure in ego functions comes from this source raises the same difficulties we have already considered in connection with exploratory behavior. That we exercise our capacities and explore our surroundings only to reduce our fear of the environment is not, as I have already argued, an assumption that enjoys high probability on biological grounds.

*Hartmann on the Ego*

A less radical change in the orthodox model is proposed by Hartmann, who, in a series of papers since 1939, often in conjunction with Kris and Loewenstein, has been refining and expanding Freud's views on the ego and the instincts. While the ego is conceived as a "substructure" of the personality, this term is somewhat metaphorical because in practice the ego has to be defined by its functions. The list of functions, which includes grasping, crawling, walking, perceiving, remembering, language, thinking, and intention, covers much the same ground that was indicated by Hendrick, but Hartmann does not attribute their growth to an instinct. On the other hand, Hartmann (1950) early came to the conclusion that development could not be explained, as Freud had seemed to conceive it, simply as a consequence of conflict between instinctual needs and frustrating realities. The instincts alone would never guarantee survival; they require mediation by the innate ego apparatus if they are to meet "the average expectable environmental conditions." He therefore proposed that we conceive of an autonomous factor in ego development, an independent maturation of functions taking place in a "conflict-free ego sphere." Functions such as locomotion ripen through maturation and through learning even when they are not caught up in struggles to obtain erotic and aggressive gratification or to avoid anxiety. As Anna Freud (1952) has pointed out, walking becomes independent of instinctual upheavals a few weeks after its beginning; thereafter, it serves the child impartially in situations of conflict and those that are free from conflict.

Hartmann's idea of autonomous ego development has of course been as-

sumed all along by workers in child psychology, but it is an important step to relate it to Freud's disclosures concerning unconscious motivation. In what now looks like an excess of enthusiasm for his own concepts, Freud (1925a) undertook to explain the outgrowing of the pleasure principle and the substituting of the reality principle as a simple and direct consequence of the frustration of instinctual needs. However, the reality principle contained the idea of postponing an immediate gratification in favor of a future one, and Hartmann (1956) properly notes that the capacities for postponement and anticipation cannot be conjured into existence simply by the collision of frustrating reality and ungratified need. Important as frustrations may be, these capacities must already be available, "some preparedness for dealing with reality" must already exist, before the frustration can produce its momentous educative effect. It can be seen from this example that Hartmann's analysis opens the way for profitable commerce between developmental psychologies inside and outside of psychoanalysis.

Hartmann's emphasis on adaptation permits him to perceive much more that is autonomous about the ego than was ever seriously included in Freud's systematic thought. He allows, for instance, that aims and interests which develop in the beginning as defenses against instincts may later become part of conflict-free spheres of activity—become interests in their own right—and thus achieve "secondary autonomy," a concept very close to Allport's (1937) functional autonomy of motives (Hartmann, 1950). He deals with the possibility that adaptive skills developing in the conflict-free sphere may have a decisive influence on the handling of conflicts. These skills have a history of their own, shaped jointly

by the child's abilities and by the responses evoked from parents. As Monroe (1955) has expressed it, they have "a very important role in the development of the conscious and semi-conscious psychological self." They may thus have a direct influence upon the outcome when a child becomes involved in conflict. Rapaport (1958) sees Hartmann's ideas on the autonomy of the ego as vital to the proper understanding not only of healthy development but also of psychopathology itself.

In explaining the autonomous growth of the ego, Hartmann makes generous use of the concept of maturation, but he naturally does not exclude learning. Hartmann (1950) entertains the possibility, mentioned casually from time to time by Freud (1916, 1949), that ego functions are supplied with their own sources of energy independent of instincts, and that there is pleasure connected with their mere exercise. However, he makes little systematic use of this idea, relying instead upon a concept more central in Freud's thinking, that of the neutralization of drive energies. Freud (1927) found that he could "make no headway" in accounting for the varied activities of the ego without assuming "a displaceable energy, which is in itself neutral, but is able to join forces either with an erotic or with a destructive impulse, differing qualitatively as they do, and augment its total cathexis." He speculated that the neutral energy came from Eros and could be conceived as desexualized libido. Hartmann, Kris, and Loewenstein (1949) carried the idea forward a logical step by proposing that the energies of aggressive instincts could similarly be neutralized and placed at the disposal of the ego. Neutralized energy contributes to the development of the ego and makes possible a continuing interest in the ob-

jects of the environment regardless of their immediate relation to erotic or aggressive needs. Hartmann (1955) finds this concept particularly helpful in unscrambling the confusions that have arisen over the concept of sublimation.

The doctrine of neutralized instinctual energies is a curious one, and we should bear in mind the complex clinical findings that perhaps suggested it. Freud was an unquestioned genius in detecting the subtle operation of erotic urges and aggressive fantasies, along with elaborate mechanisms of defense, behind the seemingly "objective" or "neutral" activities of everyday life. Remarkable transformations of interest could sometimes be observed in the course of development. For example, a patient's childhood erotic rivalry and aggressive competition with his father might later disappear beneath a strong objective interest in running the family business; then suddenly, on the brink of success, this interest might come to a total halt, paralyzed by anxiety because the underlying instinctual goals came too close to symbolic fulfillment. The reappearance of instinctual preoccupations in such a case lends a certain color to the idea that they have somehow been driving the behavior all the time, even though the daily pursuit of business goals seems utterly remote from instinctual gratifications.

It is worth noticing that Freud's procedure in making the assumption of neutralized instinctual energy is similar to the one followed by orthodox behaviorists in connection with primary drives. These theorists started from the assumption that all behavior was powered by a limited number of organic drives, and then, in order to protect this assumption, they developed further hypotheses, such as secondary reinforcement, to account for motivated behavior that bore no obvious



relation to primary goals. At the point where he could "make no headway" without postulating neutralization, Freud could conceivably have made a good deal of headway if he had been willing to assume that neutral energy, neither sexual nor aggressive, was available as a natural endowment in the first place. But he preferred to protect his assumption of two primary drives and to interpret other energies as transformations of these drives. Even so, the concept seems superfluous if we take Freud at his word about the nature of the life instincts. Freud (1949) made it clear that Eros included more than instincts having a sexual aim; its larger goal was "to establish even greater unities and to preserve them thus—in short, to bind together." Under this formula, it would seem possible to include energies inherently directed toward building up the integrated functions of the ego. But Freud did not exploit the full range of his theory of Eros and proposed only that neutral energies should be conceived as desexualized.

The concept of neutralization has in some respects had a good effect on psychoanalytic ego psychology. In Hartmann's writings, as we have seen, and in Rapaport's (1951, 1954) work on thinking, it has encouraged a strong interest in autonomous ego functions and a fresh analysis of their place in personality. Nevertheless, it seems to me an awkward conceptualization, one which in the end is likely to lead, as Colby (1955) has expressed it, to a "metapsychological snarl." The theory requires that instinctual energies can completely change their aims, which makes one wonder what purpose was served in the first place by defining them as having aims. It preserves an image of mobility of energies that seems much out of line with recent research on animal motivation, where

energy is being conceived in a constantly closer relation to specific structures. To my mind it thus compares unfavorably with its quite straightforward alternative, which is that the alleged neutralized energies are there in the first place as part of the natural make-up of an adaptive organism. I shall later develop this possibility by means of the concept of competence in its motivational aspect, and I believe that this concept gains support from certain other lines of work in the psychoanalytic tradition.

### *Motility and a Sense of Industry*

The trend away from instinct orthodoxy is illustrated by the work of Kardiner (1947) on what he calls "the development of the effective ego." Kardiner's reflections arose from his work on the traumatic neuroses of war. In these disorders the main threat is to self-preservation, and some of the most important symptoms, such as defensive rituals and paralyzes, are lodged in the action systems that normally bring about successful adaptive behavior. It thus becomes pertinent to study the growth of action systems, to discover how they become integrated so as to maintain "controlled contact" with the environment and "controlled exploitation of objects in the outer world," and to work out the conditions which either favor or disrupt this acquired integration. Thinking along these lines, Kardiner is led to conclusions just about the opposite of Freud's: It is the successful and gratifying experiences, not the frustrations, that lead to increasingly integrated action and to the discrimination of self from outer world. Frustration produces chiefly disruptions and inhibitions which are unfavorable to the early growth of the ego. Children are gratified when they discover the connection between a movement executed



and the accompanying and subsequent sensations. They are still more gratified when they carry out actions successfully; this "gives rise to the triumphant feeling of making an organ obedient to the will of the ego." Such experiences build up "a definite self- or body-consciousness which becomes the center and the point of reference of all purposeful and coordinated activity." Growth of the ego, in short, depends heavily upon action systems and the consequences of action. The course and vicissitudes of this development have to be studied in their own right, and they cannot be understood as side effects of the stages of libidinal development.

A similar theme is pursued to even more radical conclusions by Mittelmann (1954) in his paper on motility. Mittelmann regards motility, which manifests itself most typically in skilled motor actions such as posture, locomotion, and manipulation, as an "urge in its own right" in the same sense that one speaks of oral, excretory, or genital urges. From about 10 months of age it has a distinctly "driven" character, and there is restlessness and anger if it is blocked. During the second and third years the motor urge "dominates all other urges," so that it is proper to "consider this period the motor level of ego and libido development." The child makes tremendous efforts to learn to walk, and to walk well, and he exhibits joyous laughter as he attains these ends. Restrictions of motility may occur because the parents are anxious or because the child's assertiveness troubles them, and a lasting injury to the parent-child relationship may result. Clumsiness in motor or manipulative accomplishments may lead to self-hatred and dependence, for "the evolution of self-assertiveness and self-esteem is intimately connected with motor development." Motility is

of central importance in many of the most characteristic functions of the ego. Partly by its means the infant differentiates himself from other objects, and the child's knowledge of objects depends on an extensive activity of manipulation and examination. "Thus motility becomes one of the most important aspects of reality testing." Because it is an element in all cognitive behavior, it can also be considered "the dominant integrative function." Mittelmann bases motor development, in short, on an independent urge, and he sees this urge as the really crucial motive behind the development of the ego.

Like Kardiner, Mittelmann does not attempt to formulate in detail the nature of the motility urge. It is likened not to an instinct but to a "partial instinct," and this seems to place it somewhere between Hendrick's instinct to master and Hartmann's dimly sketched independent energies of the ego. This indefiniteness may irk the systematic theorist, but Mittelmann's account of the part played by motility in ego development easily stands as a significant contribution. Even more influential in this respect is the work of Erikson (1953), who has given a highly detailed timetable of ego development. Erikson stays with the libido theory as far as it will go, but he passes beyond its reach in his account of the latency period and some of the later crises of growth. It is clear that something more than the orthodox instincts is involved in the "enormous value" with which the child in the second year "begins to endow his autonomous will." Something more would seem to be implied in the expanding imagination and initiative of the "phallic" child. Certainly more is involved during the school years, when children address themselves to motor, manual, and intellectual achievements and need "a

sense of being able to make things and make them well and even perfectly: this is what I call the *sense of industry*." Erikson's (1952) theory of play is also influenced by the idea that learning to deal with the animate and inanimate worlds is an important preoccupation of childhood: "the playing child advances forward to new stages of real mastery." Action systems, motility, and a sense of industry all direct our attention to behavior which can scarcely be contained in the old bottle of instinct theory.

Glancing back over these trends in psychoanalytic ego psychology, we cannot fail to be impressed by striking similarities to the trend in animal work. Using Reik's familiar metaphor, we might say that those who listen with their two ears and those who listen with the third ear have apparently been hearing much the same sounds. In both realms there is discontent with drive orthodoxy. In both there is persistent pointing to kinds of behavior neglected or explained away by drive orthodoxy: exploration, activity, manipulation, and mastery. Similar theories have been proposed to account for the energies in such behavior: (a) they are derived or transformed in some way from the primary drives or instincts (secondary reinforcement, neutralization of drive energies); (b) they are powered by the need to reduce anxiety; (c) they can be accounted for only by postulating a new primary drive (exploratory drive, instinct to master). When these explanations are considered to have failed, the one remaining course is to work out a different idea of motivation. In his study of action systems, Kardiner prefers to leave the question of energy sources unanswered, but Erikson's sense of industry and Mittelmann's motility urge point to a motivational base which is only remotely

analogous to primary drives or fundamental instincts. I believe that the difficulties in this undertaking can be greatly reduced by the concept of competence, to which we shall shortly turn.

#### RELATED DEVELOPMENTS IN GENERAL PSYCHOLOGY

If a systematic survey were in order, it would be easy to show a parallel drift of opinion in other parts of the psychological realm. Among theorists of personality, for example, something like drive orthodoxy is to be found in the work of Dollard and Miller (1950), who have translated the main concepts of Freud's psychoanalysis, including processes such as repression and displacement, into the language of reinforcement theory. With them we might put Mowrer (1950), whose searching analysis of fear as an acquired drive has led him to postulate anxiety-reduction as the master motive behind the development of the ego. Discontent with drive orthodoxy has long been expressed by Allport (1937, 1946), who not only argues for a functional autonomy of motives from their infantile roots in primary drives but also seriously questions the law of effect, the very cornerstone of reinforcement theory. Little comfort for the orthodox can be found in Murray's (1938) detailed taxonomy of needs, especially when it comes to needs such as achievement and construction, which can be tied to primary drives only by conceptual acrobatics. Murray and Kluckhohn (1953), moreover, have made a case for pleasure in activity for its own sake, reviving the *Funktionslust* proposed many years ago by Karl Bühler (1924) and recently developed in some detail by French (1952). They also argue for intrinsic mental needs: "the infant's mind is not acting most of the time as the instrument of some urgent animal drive, but is pre-

occupied with *gratifying itself*." Murphy (1947) takes the view that all tissues can become seats of tension and thus participants in drive; in addition to visceral drives, he postulates two independent forms, activity drives and sensory drives. Then there are workers such as Goldstein (1939) who approach the whole problem with a holistic philosophy which precludes the dictatorship of any isolated or partial drives. Goldstein (1940) assumes one master tendency, that toward self-actualization, of which the so-called visceral drives are but partial and not really isolated expressions, and which can find expression also in an urge toward perfection—toward completing what is incomplete, whether it be an outside task or the mastery of some function such as walking. It has been shown by the Ansbachers (1956) that Adler, never a friend of instinct orthodoxy, in his later years reached an idea very similar to the urge toward perfection. Maslow (1954, 1955), too, belongs with the heterodox. He insists that we should take account of growth motivation as well as the deficiency motivation implied in the visceral drives, and he offers the valuable idea of a hierarchy of motives, according to which the satisfaction of "lower" needs makes it possible for "higher" needs to emerge and become regnant in behavior.

Mention of these names must suffice here to show that the trends observed in animal psychology and psychoanalytic ego psychology are pervasive in contemporary psychological thought. Doubtless the same controversies and problems could be pointed out in child development, in cognitive psychology, and in other fields. But in order to advance to my main theme, I shall select only certain developments which bear directly on the concept of competence.

### *Needs for Excitement and Novelty*

Human experience provides plentiful evidence of the importance of reducing excessive levels of tension. Men under wartime stress, men under pressure of pain and extreme deprivation, men with excessive work loads or too much exposure to confusing social interactions, all act as if their nervous systems craved that utterly unstimulated condition which Freud once sketched as the epitome of neural bliss. But if these same men be granted their Nirvana they soon become miserable and begin to look around for a little excitement. Human experience testifies that boredom is a bad state of affairs about which something must be done. Hebb (1949) has been particularly insistent in reminding us that many of our activities, such as reading detective stories, skin-diving, or driving cars at high speeds, give clear evidence of a need to raise the level of stimulation and excitement. Men and animals alike seem at times bent on increasing the impact of the environment and even on creating mild degrees of frustration and fear. Hebb and Thompson (1954) reflect upon this as follows:

Such phenomena are, of course, well known in man: in the liking for dangerous sports or roller coasters, where fear is deliberately courted, and in the addiction to bridge or golf or *solitaire*, vices whose very existence depends upon the level of difficulty of the problems presented and an optimal level of frustration. Once more, when we find such attitudes toward fear and frustration in animals, we have a better basis for supposing that we are dealing with something fundamental if a man prefers skis to the less dangerous snowshoes, or when we observe an unashamed love of work (problem solving and frustration included) in the scientist, or in the business man who cannot retire. Such behavior in man is usually accounted for as a search for prestige, but the animal data make this untenable. It seems much more likely that solving problems and running mild risks are inherently

rewarding, or, in more general terms, that the animal will always act so as to produce an optimal level of excitation (Hebb & Thompson, 1954, p. 551).

The concept of optimal stimulation has been developed by Leuba (1955), who sees it as helpful in resolving some of the problems of learning theory. Believing that most theorizing about motivation has been based upon "powerful biological or neurotic drives," Leuba bids us look at the much more common learning situations of nursery, playground, and school, where "actions which increase stimulation and produce excitement are strongly reinforced, sometimes to the dismay of parents and teachers." He proposes that there is an optimal level of stimulation, subject to variation at different times, and that learning is associated with movement toward this optimal level, downward when stimulation is too high and upward when it is too low. A similar idea is expressed by McReynolds (1956) concerning the more restricted concept of "rate of perceptualization." Monotonous conditions provide too low a rate, with boredom; excessive stimulation produces too high a rate, with disruptive excitement; the optimal rate yields the experience of pleasure. These ideas are now amply supported by recent experimental work on sensory deprivation (Lilly, 1956; Hebb, 1958).

In recent papers Young (1949, 1955) has argued for an hedonic theory of motivation, one in which affective processes "constitute a form of primary motivation." According to Young's theory, "an organism behaves so as to maximize positive affective arousal (delight, enjoyment) and to minimize negative arousal (distress)." McClelland (1953) has offered a version of hedonic theory which is of particular value in understanding the significance of novelty. Affective arousal

occurs when a stimulus pattern produces a discrepancy from the existing adaptation level. Small discrepancies produce pleasant affect and a tendency to approach; large ones produce unpleasantness and a tendency toward avoidance. The child at play, like the young chimpanzee and the exploring rat, needs frequent novelty in the stimulus field in order to keep up his interest—in order to maintain pleasant discrepancies from whatever adaptation level he has reached. Hebb's (1949) theory of the neurological correlates of learning also deals with novelty, though in a somewhat different way. He equates sustained interest with a state of neural affairs in which "phase sequences" are relatively complex and are growing, in the sense of establishing new internal relations. Such a state follows most readily from a stimulus field characterized by difference-in-sameness; that is, containing much that is familiar along with certain features that are novel. If the field is entirely familiar, phase sequences run off quickly, are short-circuited, and thus fail to produce sustained interest. Hebb's theory, which has the engaging quality of being able to explain why we enjoy reading a detective story once but not right over again, expresses in a neurological hypothesis the familiar fact that well-learned, habituated processes do not in themselves greatly interest us. Interest seems to require elements of unfamiliarity: of something still to be found out and of learning still to be done.

It seems to me that these contributions, though differing as to details, speak with unanimity on their central theme and would force us, if nothing else did, to reconsider seriously the whole problem of motivation. Boredom, the unpleasantness of monotony, the attraction of novelty, the tendency

to vary behavior rather than repeating it rigidly, and the seeking of stimulation and mild excitement stand as inescapable facts of human experience and clearly have their parallels in animal behavior. We may seek rest and minimal stimulation at the end of the day, but that is not what we are looking for the next morning. Even when its primary needs are satisfied and its homeostatic chores are done, and organism is alive, active, and up to something.

### *Dealing with the Environment*

If we consider things only from the viewpoint of affect, excitement, and novelty, we are apt to overlook another important aspect of behavior, its effect upon the environment. Moving in this direction, Diamond (1939) invites us to consider the motivational properties of the sensorineural system, the apparatus whereby higher animals "maintain their relations to the environment." He conceives of this system as demanding stimulation and as acting in such a manner as to "force the environment to stimulate it." Even if one thinks only of the infant's exploring eyes and hands, it is clear that the main direction of behavior is by no means always that of reducing the impact of stimulation. When the eyes follow a moving object, or when the hand grasps an object which it has touched, the result is to preserve the stimulus and to increase its effect. In more elaborate explorations the consequence of a series of actions may be to vary the manner in which a stimulus acts upon the sense organs. It is apparent that the exploring, manipulating child produces by his actions precisely what Hebb's theory demands as a basis for continuing interest: he produces differences-in-sameness in the stimulus field.

In a critical analysis of Freud's

views on the reality principle, Charlotte Bühler (1954) makes a strong case for positive interests in the environment, citing as evidence the responsiveness and adaptiveness of the newborn baby as well as the exploratory tendencies of later months. The problem is worked out in more detail by Schachtel (1954) in a paper on focal attention. Acts of focal attention are characteristically directed at particular objects, and they consist of several sustained approaches "aimed at active mental grasp" while excluding the rest of the field. These qualities can be observed even in the infant's early attempts to follow a moving object with his eyes, and they show more clearly in his later endeavors to learn how objects are related both to himself and to one another. Such behavior bespeaks "a relatively autonomous capacity for object interest." Schachtel makes the proposal that this interest is pursued precisely at those times when major needs are in abeyance. High pressure of need or anxiety is the enemy of exploratory play and is a condition, as every scientist should know, under which we are unlikely to achieve an objective grasp of the environment. Low need pressure is requisite if we are to perceive objects as they are, in their constant character, apart from hopes and fears we may at other times attach to them. Schachtel doubts that "the wish for need-satisfaction alone would ever lead to object perception and to object-oriented thought." Hence an autonomous capacity to be interested in the environment has great value for the survival of a species.

Being interested in the environment implies having some kind of satisfactory interaction with it. Several workers call attention to the possibility that satisfaction might lie in having an effect upon the environment, in dealing



with it, and changing it in various ways. Groos (1901), in his classical analysis of play, attached great importance to the child's "joy in being a cause," as shown in making a clatter, "hustling things about," and playing in puddles where large and dramatic effects can be produced. "We demand a knowledge of effects," he wrote, "and to be ourselves the producers of effects." Piaget (1952) remarks upon the child's special interest in objects that are affected by his own movements. This aspect of behavior occupies a central place in the work of Skinner (1953), who describes it as "operant" and who thus "emphasizes the fact that the behavior *operates* upon the environment to generate consequences." These consequences are fed back through the sense organs and may serve to reinforce behavior even when no organic needs are involved. A rat will show an increased tendency to press a bar when this act produces a click or a buzz. A baby will continue to investigate when his efforts produce rattling or tinkling sounds or sparkling reflections from a shiny object. The young chimpanzees in Welker's experiment spent the longest time over objects which could be lighted or made to emit sounds. Skinner finds it "difficult, if not impossible, to trace these reinforcing effects to a history of conditioning." "We may plausibly argue," he continues, "that a capacity to be reinforced by any feedback from the environment would be biologically advantageous, since it would prepare the organism to manipulate the environment successfully before a given state of deprivation developed."

#### *Woodworth's Behavior-Primacy Theory*

The most far-reaching attempt to give these aspects of behavior a sys-

tematic place in the theory of motivation is contained in Woodworth's recent book, *Dynamics of Behavior* (1958). Woodworth takes his start from the idea that a great deal of human behavior appears to be directed toward producing effects upon the environment without immediate service to any aroused organic need. "Its incentives and rewards are in the field of behavior and not in the field of homeostasis." This is illustrated by exploratory behavior, which is directed outward toward the environment.

Its long-range value as the means of making the child acquainted with the world he has to deal with later, and so equipping him through play for the serious business of life, can scarcely lie within the little child's horizon. His goals are more limited and direct: to see this or that object more closely, to find what is behind an obstacle, to hear the noise an object makes when it strikes the floor, to be told the name of a thing or person (Woodworth, 1958, p. 78).

More complex play, such as building with blocks, illustrates the same outgoing tendency and reveals more plainly the element of finding out what one can and cannot do with objects. Even social play falls into the pattern. Playmates do not chiefly supply affection or satisfy organic needs; rather, they "afford the opportunity to do something interesting in the environment."

Woodworth draws a contrast between *need-primacy* theories of motivation and the *behavior-primacy* theory. The latter holds that "all behavior is directed primarily toward dealing with the environment." It is to be noted that "dealing with the environment" means a good deal more than receiving stimuli and making responses. Stimuli must be taken as indicators of objects in space, and responses must be adapted to produce effects upon these objects. Even the so-called "mental" capacities, such as memory and ideational think-



ing, become in time high-level methods of dealing with the environment. Woodworth leaves no doubt as to what he considers basic in motivation. "We are making the claim that this direction of receptive and motor activity toward the environment is the fundamental tendency of animal and human behavior and that it is the all-pervasive primary motivation of behavior." Organic drives have to break into this constantly flowing stream of activity and turn it in a special direction. But the goals of drives cannot be achieved without effective action upon one's surroundings. The ever-present, ever-primary feature of motivation is the tendency to deal with the environment.

It may appear to some workers that Woodworth has overshot the mark by making primary what has commonly been regarded as secondary, and by reducing the familiar drives to what sounds a little like a subordinate station. Woodworth's theory, however, like Goldstein's concept of self-actualization, probably should be construed not as an attempt to down-grade the drives but rather as an insistence that they be kept in the context of a whole living organism which during its waking hours is more or less constantly active. Woodworth's emphasis on dealing with the environment makes his theory a point of culmination for many of those driftings away from drive orthodoxy which we have found to be persistent in so many different areas of psychology. It will soon appear that the concept of competence, to which I now turn, represents in many respects a similar way of thinking. It emphasizes dealing with the environment, and it belongs in the trend away from drive *orthodoxy*, but it is not intended to supplant, or even to subsume, such dynamic forces as hunger, sex, aggression, and fear, which everyone knows to be of huge

importance in animal and human nature.

#### COMPETENCE AND THE PLAY OF CONTENTED CHILDREN

A backward glance at our survey shows considerable agreement about the kinds of behavior that are left out or handled poorly by theories of motivation based wholly on organic drives. Repeatedly we find reference to the familiar series of learned skills which starts with sucking, grasping, and visual exploration and continues with crawling and walking, acts of focal attention and perception, memory, language and thinking, anticipation, the exploring of novel places and objects, effecting stimulus changes in the environment, manipulating and exploiting the surroundings, and achieving higher levels of motor and mental coordination. These aspects of behavior have long been the province of child psychology, which has attempted to measure the slow course of their development and has shown how heavily their growth depends upon learning. Collectively they are sometimes referred to as adaptive mechanisms or as ego processes, but on the whole we are not accustomed to cast a single name over the diverse feats whereby we learn to deal with the environment.

I now propose that we gather the various kinds of behavior just mentioned, all of which have to do with effective interaction with the environment, under the general heading of competence. According to Webster, competence means fitness or ability, and the suggested synonyms include capability, capacity, efficiency, proficiency, and skill. It is therefore a suitable word to describe such things as grasping and exploring, crawling and walking, attention and perception, language and thinking, manipulating and changing the surroundings, all of which

promote an effective—a competent—interaction with the environment. It is true, of course, that maturation plays a part in all these developments, but this part is heavily overshadowed by learning in all the more complex accomplishments like speech or skilled manipulation. I shall argue that it is necessary to make competence a motivational concept; there is a *competence motivation* as well as competence in its more familiar sense of achieved capacity. The behavior that leads to the building up of effective grasping, handling, and letting go of objects, to take one example, is not random behavior produced by a general overflow of energy. It is directed, selective, and persistent, and it is continued not because it serves primary drives, which indeed it cannot serve until it is almost perfected, but because it satisfies an intrinsic need to deal with the environment.

No doubt it will at first seem arbitrary to propose a single motivational conception in connection with so many and such diverse kinds of behavior. What do we gain by attributing motivational unity to such a large array of activities? We could, of course, say that each developmental sequence, such as learning to grasp or to walk, has its own built-in bit of motivation—its “aliment,” as Piaget (1952) has expressed it. We could go further and say that each item of behavior has its intrinsic motive—but this makes the concept of motivation redundant. On the other hand, we might follow the lead of the animal psychologists and postulate a limited number of broader motives under such names as curiosity, manipulation, and mastery. I believe that the idea of a competence motivation is more adequate than any of these alternatives and that it points to very vital common properties which have been lost from view amidst the strongly

analytical tendencies that go with detailed research.

In order to make this claim more plausible, I shall now introduce some specimens of playful exploration in early childhood. I hope that these images will serve to fix and dramatize the concept of competence in the same way that other images—the hungry animal solving problems, the child putting his finger in the candle flame, the infant at the breast, the child on the toilet, and the youthful Oedipus caught in a hopeless love triangle—have become memorable focal points for other concepts. For this purpose I turn to Piaget's (1952) studies of the growth of intelligence from its earliest manifestations in his own three children. The examples come from the first year of life, before language and verbal concepts begin to be important. They therefore represent a practical kind of intelligence which may be quite similar to what is developed by the higher animals.

As early as the fourth month, the play of the gifted Piaget children began to be “centered on a result produced in the external environment,” and their behavior could be described as re-discovering the movement which by chance exercised an advantageous action upon things” (1952, p. 151). Laurent, lying in his bassinet, learns to shake a suspended rattle by pulling a string that hangs from it. He discovers this result fortuitously before vision and prehension are fully coordinated. Let us now observe him a little later when he has reached the age of three months and ten days.

I place the string, which is attached to the rattle, in his right hand, merely unrolling it a little so that he may grasp it better. For a moment nothing happens. But at the first shake due to chance movement of his hand, the reaction is immediate: Laurent starts when looking at the rattle and then violently strikes his right hand alone, as if

he felt the resistance and the effect. The operation lasts fully a quarter of an hour, during which Laurent emits peals of laughter (Piaget, 1952, p. 162).

Three days later the following behavior is observed.

Laurent, by chance, strikes the chain while sucking his fingers. He grasps it and slowly displaces it while looking at the rattles. He then begins to swing it very gently, which produces a slight movement of the hanging rattles and an as yet faint sound inside them. Laurent then definitely increases by degrees his own movements. He shakes the chain more and more vigorously and laughs uproariously at the result obtained. (Piaget, 1952, p. 185).

Very soon it can be observed that procedures are used "to make interesting spectacles last." For instance, Laurent is shown a rubber monkey which he has not seen before. After a moment of surprise, and perhaps even fright, he calms down and makes movements of pulling the string, a procedure which has no effect in this case, but which previously has caused interesting things to happen. It is to be noticed that "interesting spectacles" consist of such things as new toys, a tin box upon which a drumming noise can be made, an unfolded newspaper, or sounds made by the observer such as snapping the fingers. Commonplace as they are to the adult mind, these spectacles enter the infant's experience as novel and apparently challenging events.

Moving ahead to the second half of the first year, we can observe behavior in which the child explores the properties of objects and tries out his repertory of actions upon them. This soon leads to active experimentation in which the child attempts to provoke new results. Again we look in upon Laurent, who has now reached the age of nine months. On different occasions he is shown a variety of new objects—for instance a notebook, a

beaded purse, and a wooden parrot. His carefully observing father detects four stages of response: (a) visual exploration, passing the object from hand to hand, folding the purse, *etc.*; (b) tactile exploration, passing the hand all over the object, scratching, *etc.*; (c) slow moving of the object in space; (d) use of the repertory of action: shaking the object, striking it, swinging it, rubbing it against the side of the bassinet, sucking it, *etc.*, "each in turn with a sort of prudence as though studying the effect produced" (1952, p. 255).

Here the child can be described as applying familiar tactics to new situations, but in a short while he will advance to clear patterns of active experimentation. At 10 months and 10 days Laurent, who is unfamiliar with bread as a nutritive substance, is given a piece for examination. He manipulates it, drops it many times, breaks off fragments and lets them fall. He has often done this kind of thing before, but previously his attention has seemed to be centered on the act of letting go. Now "he watches with great interest the body in motion; in particular, he looks at it for a long time when it has fallen, and picks it up when he can." On the following day he resumes his research.

He grasps in succession a celluloid swan, a box, and several other small objects, in each case stretching out his arm and letting them fall. Sometimes he stretches out his arm vertically, sometimes he holds it obliquely in front of or behind his eyes. When the object falls in a new position (for example on his pillow) he lets it fall two or three times more on the same place, as though to study the spatial relation; then he modifies the situation. At a certain moment the swan falls near his mouth; now he does not suck it (even though this object habitually serves this purpose), but drops it three times more while merely making the gesture of opening his mouth (Piaget, 1952, p. 269).

These specimens will furnish us with sufficient images of the infant's use of his spare time. Laurent, of course, was provided by his studious father with a decidedly enriched environment, but no observant parent will question the fact that babies often act this way during those periods of their waking life when hunger, erotic needs, distresses, and anxiety seem to be exerting no particular pressure. If we consider this behavior under the historic headings of psychology we shall see that few processes are missing. The child gives evidence of sensing, perceiving, attending, learning, recognizing, probably recalling, and perhaps thinking in a rudimentary way. Strong emotion is lacking, but the infant's smiles, gurgles, and occasional peals of laughter strongly suggest the presence of pleasant affect. Actions appear in an organized form, particularly in the specimens of active exploration and experimentation. Apparently the child is using with a certain coherence nearly the whole repertory of psychological processes except those that accompany stress. It would be arbitrary indeed to say that one was more important than another.

These specimens have a meaningful unity when seen as transactions between the child and his environment, the child having some influence upon the environment and the environment some influence upon the child. Laurent appears to be concerned about what he can do with the chain and rattles, what he can accomplish by his own effort to reproduce and to vary the entertaining sounds. If his father observed correctly, we must add that Laurent seems to have varied his actions systematically, as if testing the effect of different degrees of effort upon the bit of environment represented by the chain and rattles. Kit-

tens make a similar study of parameters when delicately using their paws to push pencils and other objects ever nearer to the edge of one's desk. In all such examples it is clear that the child or animal is by no means at the mercy of transient stimulus fields. He selects for continuous treatment those aspects of his environment which he finds it possible to affect in some way. His behavior is selective, directed, persistent—in short, motivated.

Motivated toward what goal? In these terms, too, the behavior exhibits a little of everything. Laurent can be seen as appeasing a stimulus hunger, providing his sensorium with an agreeable level of stimulation by eliciting from the environment a series of interesting sounds, feels, and sights. On the other hand we might emphasize a need for activity and see him as trying to reach a pleasurable level of neuromuscular exercise. We can also see another possible goal in the behavior: the child is achieving knowledge, attaining a more differentiated cognitive map of his environment and thus satisfying an exploratory tendency or motive of curiosity. But it is equally possible to discern a theme of mastery, power, or control, perhaps even a bit of primitive self-assertion, in the child's concentration upon those aspects of the environment which respond in some way to his own activity. It looks as if we had found too many goals, and perhaps our first impulse is to search for some key to tell us which one is really important. But this, I think, is a mistake that would be fatal to understanding.

We cannot assign priority to any of these goals without pausing arbitrarily in the cycle of transaction between child and environment and saying, "This is the real point." I propose instead that the real point is the trans-

actions as a whole. If the behavior gives satisfaction, this satisfaction is not associated with a particular moment in the cycle. It does not lie solely in sensory stimulation, in a bettering of the cognitive map, in coordinated action, in motor exercise, in a feeling of effort and of effects produced, or in the appreciation of change brought about in the sensory field. These are all simply aspects of a process which at this stage has to be conceived as a whole. The child appears to be occupied with the agreeable task of developing an effective familiarity with his environment. This involves discovering the effects he can have on the environment and the effects the environment will have on him. To the extent that these results are preserved by learning, they build up an increased competence in dealing with the environment. The child's play can thus be viewed as serious business, though to him it is merely something that is interesting and fun to do.

Bearing in mind these examples, as well as the dealings with environment pointed out by other workers, we must now attempt to describe more fully the possible nature of the motivational aspect of competence. It needs its own name, and in view of the foregoing analysis I propose that this name be *effectance*.

#### EFFECTANCE

The new freedom produced by two decades of research on animal drives is of great help in this undertaking. We are no longer obliged to look for a source of energy external to the nervous system, for a consummatory climax, or for a fixed connection between reinforcement and tension-reduction. Effectance motivation cannot, of course, be conceived as having a source in tissues external to the ner-

vous system. It is in no sense a deficit motive. We must assume it to be neurogenic, its "energies" being simply those of the living cells that make up the nervous system. External stimuli play an important part, but in terms of "energy" this part is secondary, as one can see most clearly when environmental stimulation is actively sought. Putting it picturesquely, we might say that the effectance urge represents what the neuromuscular system wants to do when it is otherwise unoccupied or is gently stimulated by the environment. Obviously there are no consummatory acts; satisfaction would appear to lie in the arousal and maintaining of activity rather than in its slow decline toward bored passivity. The motive need not be conceived as intense and powerful in the sense that hunger, pain, or fear can be powerful when aroused to high pitch. There are plenty of instances in which children refuse to leave their absorbed play in order to eat or to visit the toilet. Strongly aroused drives, pain, and anxiety, however, can be conceived as overriding the effectance urge and capturing the energies of the neuromuscular system. But effectance motivation is persistent in the sense that it regularly occupies the spare waking time between episodes of homeostatic crisis.

In speculating upon this subject we must bear in mind the continuous nature of behavior. This is easier said than done; habitually we break things down in order to understand them, and such units as the reflex arc, the stimulus-response sequence, and the single transaction with the environment seem like inevitable steps toward clarity. Yet when we apply such an analysis to playful exploration we lose the most essential aspect of the behavior. It is constantly circling from stimulus to perception to action to ef-



fect to stimulus to perception, and so on around; or, more properly, these processes are all in continuous action and continuous change. Dealing with the environment means carrying on a continuing transaction which gradually changes one's relation to the environment. Because there is no consummatory climax, satisfaction has to be seen as lying in a considerable series of transactions, in a trend of behavior rather than a goal that is achieved. It is difficult to make the word "satisfaction" have this connotation, and we shall do well to replace it by "feeling of efficacy" when attempting to indicate the subjective and affective side of effectance.

It is useful to recall the findings about novelty: the singular effectiveness of novelty in engaging interest and for a time supporting persistent behavior. We also need to consider the selective continuance of transactions in which the animal or child has a more or less pronounced effect upon the environment—in which something happens as a consequence of his activity. Interest is not aroused and sustained when the stimulus field is so familiar that it gives rise at most to reflex acts or automatized habits. It is not sustained when actions produce no effects or changes in the stimulus field. Our conception must therefore be that effectance motivation is aroused by stimulus conditions which offer, as Hebb (1949) puts it, difference-in-sameness. This leads to variability and novelty of response, and interest is best sustained when the resulting action affects the stimulus so as to produce further difference-in-sameness. Interest wanes when action begins to have less effect; effectance motivation subsides when a situation has been explored to the point that it no longer presents new possibilities.

We have to conceive further that the arousal of playful and exploratory interest means the appearance of organization involving both the cognitive and active aspects of behavior. Change in the stimulus field is not an end in itself, so to speak; it happens when one is passively moved about, and it may happen as a consequence of random movements without becoming focalized and instigating exploration. Similarly, action which has effects is not an end in itself, for if one unintentionally kicks away a branch while walking, or knocks something off a table, these effects by no means necessarily become involved in playful investigation. Schachtel's (1954) emphasis on focal attention becomes helpful at this point. The playful and exploratory behavior shown by Laurent is not random or casual. It involves focal *attention* to some object—the fixing of some aspect of the stimulus field so that it stays relatively constant—and it also involves the focalizing of *action* upon this object. As Diamond (1939) has expressed it, response under these conditions is "relevant to the stimulus," and it is change in the *focalized* stimulus that so strongly affects the level of interest. Dealing with the environment means directing focal attention to some part of it and organizing actions to have some effect on this part.

In our present state of relative ignorance about the workings of the nervous system it is impossible to form a satisfactory idea of the neural basis of effectance motivation, but it should at least be clear that the concept does not refer to any and every kind of neural action. It refers to a particular kind of activity, as inferred from particular kinds of behavior. We can say that it does not include reflexes and other kinds of automatic response. It does not include well-learned, automa-



tized patterns, even those that are complex and highly organized. It does not include behavior in the service of effectively aroused drives. It does not even include activity that is highly random and discontinuous, though such behavior may be its most direct forerunner. The urge toward competence is inferred specifically from behavior that shows a lasting focalization and that has the characteristics of exploration and experimentation, a kind of variation within the focus. When this particular sort of activity is aroused in the nervous system, effectance motivation is being aroused, for it is characteristic of this particular sort of activity that it is selective, directed, and persistent, and that instrumental acts will be learned for the sole reward of engaging in it.

Some objection may be felt to my introducing the word *competence* in connection with behavior that is so often playful. Certainly the playing child is doing things for fun, not because of a desire to improve his competence in dealing with the stern hard world. In order to forestall misunderstanding, it should be pointed out that the usage here is parallel to what we do when we connect sex with its biological goal of reproduction. The sex drive aims for pleasure and gratification, and reproduction is a consequence that is presumably unforeseen by animals and by man at primitive levels of understanding. Effectance motivation similarly aims for the feeling of efficacy, not for the vitally important learnings that come as its consequence. If we consider the part played by competence motivation in adult human life we can observe the same parallel. Sex may now be completely and purposefully divorced from reproduction but nevertheless pursued for the pleasure it can yield. Similarly, effectance

motivation may lead to continuing exploratory interests or active adventures when in fact there is no longer any gain in actual competence or any need for it in terms of survival. In both cases the motive is capable of yielding surplus satisfaction well beyond what is necessary to get the biological work done.

In infants and young children it seems to me sensible to conceive of effectance motivation as undifferentiated. Later in life it becomes profitable to distinguish various motives such as cognizance, construction, mastery, and achievement. It is my view that all such motives have a root in effectance motivation. They are differentiated from it through life experiences which emphasize one or another aspect of the cycle of transaction with environment. Of course, the motives of later childhood and of adult life are no longer simple and can almost never be referred to a single root. They can acquire loadings of anxiety, defense, and compensation, they can become fused with unconscious fantasies of a sexual, aggressive, or omnipotent character, and they can gain force because of their service in producing realistic results in the way of income and career. It is not my intention to cast effectance in the star part in adult motivation. The acquisition of motives is a complicated affair in which simple and sovereign theories grow daily more obsolete. Yet it may be that the satisfaction of effectance contributes significantly to those feelings of interest which often sustain us so well in day-to-day actions, particularly when the things we are doing have continuing elements of novelty.

#### THE BIOLOGICAL SIGNIFICANCE OF COMPETENCE

The conviction was expressed at the beginning of this paper that some such

concept as competence, interpreted motivationally, was essential for any biologically sound view of human nature. This necessity emerges when we consider the nature of living systems, particularly when we take a longitudinal view. What an organism does at a given moment does not always give the right clue as to what it does over a period of time. Discussing this problem, Angyal (1941) has proposed that we should look for the general pattern followed by the total organismic process over the course of time. Obviously this makes it necessary to take account of growth. Angyal defines life as "a process of self-expansion"; the living system "expands at the expense of its surroundings," assimilating parts of the environment and transforming them into functioning parts of itself. Organisms differ from other things in nature in that they are "self-governing entities" which are to some extent "autonomous." Internal processes govern them as well as external "heteronomous" forces. In the course of life there is a relative increase in the preponderance of internal over external forces. The living system expands, assimilates more of the environment, transforms its surroundings so as to bring them under greater control. "We may say," Angyal writes, "that the general dynamic trend of the organism is toward an increase of autonomy. . . . The human being has a characteristic tendency toward self-determination, that is, a tendency to resist external influences and to subordinate the heteronomous forces of the physical and social environment to its own sphere of influence." The trend toward increased autonomy is characteristic so long as growth of any kind is going on, though in the end the living system is

bound to succumb to the pressure of heteronomous forces.

Of all living creatures, it is man who takes the longest strides toward autonomy. This is not because of any unusual tendency toward bodily expansion at the expense of the environment. It is rather that man, with his mobile hands and abundantly developed brain, attains an extremely high level of competence in his transactions with his surroundings. The building of houses, roads and bridges, the making of tools and instruments, the domestication of plants and animals, all qualify as planful changes made in the environment so that it comes more or less under control and serves our purposes rather than intruding upon them. We meet the fluctuations of outdoor temperature, for example, not only with our bodily homeostatic mechanisms, which alone would be painfully unequal to the task, but also with clothing, buildings, controlled fires, and such complicated devices as self-regulating central heating and air conditioning. Man as a species has developed a tremendous power of bringing the environment into his service, and each individual member of the species must attain what is really quite an impressive level of competence if he is to take part in the life around him.

We are so accustomed to these human accomplishments that it is hard to realize how long an apprenticeship they require. At the outset the human infant is a slow learner in comparison with other animal forms. Hebb (1949) speaks of "the astonishing inefficiency of man's first learning, as far as immediate results are concerned," an inefficiency which he attributes to the large size of the association areas in the brain and the long time needed to

bring them under sensory control. The human lack of precocity in learning shows itself even in comparison with one of the next of kin: as Hebb points out, "the human baby takes six months, the chimpanzee four months, before making a clear distinction between friend and enemy." Later in life the slow start will pay dividends. Once the fundamental perceptual elements, simple associations, and conceptual sequences have been established, later learning can proceed with ever increasing swiftness and complexity. In Hebb's words, "learning at maturity concerns patterns and events whose parts at least are familiar and which already have a number of other associations."

This general principle of cumulative learning, starting from slowly acquired rudiments and proceeding thence with increasing efficiency, can be illustrated by such processes as manipulation and locomotion, which may culminate in the acrobat devising new stunts or the dancer working out a new ballet. It is especially vivid in the case of language, where the early mastery of words and pronunciation seems such a far cry from spontaneous adult speech. A strong argument has been made by Hebb (1949) that the learning of visual forms proceeds over a similar course from slowly learned elements to rapidly combined patterns. Circles and squares, for example, cannot be discriminated at a glance without a slow apprenticeship involving eye movements, successive fixations, and recognition of angles. Hebb proposes that the recognition of visual patterns without eye movement "is possible only as the result of an intensive and prolonged visual training that goes on from the moment of birth, during every moment that the eyes are

open, with an increase in skill evident over a period of 12 to 16 years at least."

On the motor side there is likewise a lot to be cumulatively learned. The playing, investigating child slowly finds out the relationships between what he does and what he experiences. He finds out, for instance, how hard he must push what in order to produce what effect. Here the S-R formula is particularly misleading. It would come nearer the truth to say that the child is busy learning R-S connections—the effects that are likely to follow upon his own behavior. But even in this reversed form the notion of bonds or connections would still misrepresent the situation, for it is only a rare specimen of behavior that can properly be conceived as determined by fixed neural channels and a fixed motor response. As Hebb has pointed out, discussing the phenomenon of "motor equivalence" named by Lashley (1942), a rat which has been trained to press a lever will press it with the left forepaw, the right forepaw, by climbing upon it, or by biting it; a monkey will open the lid of a food box with either hand, with a foot, or even with a stick; and we might add that a good baseball player can catch a fly ball while running in almost any direction and while in almost any posture, including leaping in the air and plunging forward to the ground. All of these feats are possible because of a history of learnings in which the main lesson has been the effects of actions upon the stimulus fields that represent the environment. What has been learned is not a fixed connection but a flexible relationship between stimulus fields and the effects that can be produced in them by various kinds of action.

One additional example, drawn this

time from Piaget (1952), is particularly worth mentioning because of its importance in theories of development. Piaget points out that a great deal of mental development depends upon the idea that the world is made up of objects having substance and permanence. Without such an "object concept" it would be impossible to build up the ideas of space and causality and to arrive at the fundamental distinction between self and external world. Observation shows that the object concept, "far from being innate or ready-made in experience, is constructed little by little." Up to 7 and 8 months the Piaget children searched for vanished objects only in the sense of trying to continue the actions, such as sucking or grasping, in which the objects had played a part. When an object was really out of sight or touch, even if only because it was covered by a cloth, the infants undertook no further exploration. Only gradually, after some study of the displacement of objects by moving, swinging, and dropping them, does the child begin to make an active search for a vanished object, and only still more gradually does he learn, at 12 months or more, to make allowance for the object's sequential displacements and thus to seek it where it has gone rather than where it was last in sight. Thus it is only through cumulative learning that the child arrives at the idea of permanent substantial objects.

The infant's play is indeed serious business. If he did not while away his time pulling strings, shaking rattles, examining wooden parrots, dropping pieces of bread and celluloid swans, when would he learn to discriminate visual patterns, to catch and throw, and to build up his concept of the object? When would he acquire

the many other foundation stones necessary for cumulative learning? The more closely we analyze the behavior of the human infant, the more clearly do we realize that infancy is not simply a time when the nervous system matures and the muscles grow stronger. It is a time of active and continuous learning, during which the basis is laid for all those processes, cognitive and motor, whereby the child becomes able to establish effective transactions with his environment and move toward a greater degree of autonomy. Helpless as he may seem until he begins to toddle, he has by that time already made substantial gains in the achievement of competence.

Under primitive conditions survival must depend quite heavily upon achieved competence. We should expect to find things so arranged as to favor and maximize this achievement. Particularly in the case of man, where so little is provided innately and so much has to be learned through experience, we should expect to find highly advantageous arrangements for securing a steady cumulative learning about the properties of the environment and the extent of possible transactions. Under these circumstances we might expect to find a very powerful drive operating to insure progress toward competence, just as the vital goals of nutrition and reproduction are secured by powerful drives, and it might therefore seem paradoxical that the interests of competence should be so much entrusted to times of play and leisurely exploration. There is good reason to suppose, however, that a strong drive would be precisely the wrong arrangement to secure a flexible, knowledgeable power of transaction with the environment. Strong drives cause us to learn certain lessons well, but they

do not create maximum familiarity with our surroundings.

This point was demonstrated half a century ago in some experiments by Yerkes and Dodson (1908). They showed that maximum motivation did not lead to the most rapid solving of problems, especially if the problems were complex. For each problem there was an optimum level of motivation, neither the highest nor the lowest, and the optimum was lower for more complex tasks. The same problem has been discussed more recently by Tolman (1948) in his paper on cognitive maps. A cognitive map can be narrow or broad, depending upon the range of cues picked up in the course of learning. Tolman suggests that one of the conditions which tend to narrow the range of cues is a high level of motivation. In everyday terms, a man hurrying to an important business conference is likely to perceive only the cues that help him to get there faster, whereas a man taking a stroll after lunch is likely to pick up a substantial amount of casual information about his environment. The latent learning experiments with animals, and experiments such as those of Johnson (1953) in which drive level has been systematically varied in a situation permitting incidental learning, give strong support to this general idea. In a recent contribution, Bruner, Matter, and Papanek (1955) make a strong case for the concept of breadth of learning and provide additional evidence that it is favored by moderate and hampered by strong motivation. The latter "has the effect of speeding up learning at the cost of narrowing it." Attention is concentrated upon the task at hand and little that is extraneous to this task is learned for future use.

These facts enable us to see the bio-

logical appropriateness of an arrangement which uses periods of less intense motivation for the development of competence. This is not to say that the narrower but efficient learnings that go with the reduction of strong drives make no contribution to general effectiveness. They are certainly an important element in capacity to deal with the environment, but a much greater effectiveness results from having this capacity fed also from learnings that take place in quieter times. It is then that the infant can attend to matters of lesser urgency, exploring the properties of things he does not fear and does not need to eat, learning to gauge the force of his string-pulling when the only penalty for failure is silence on the part of the attached rattles, and generally accumulating for himself a broad knowledge and a broad skill in dealing with his surroundings.

The concept of competence can be most easily discussed by choosing, as we have done, examples of interaction with the inanimate environment. It applies equally well, however, to transactions with animals and with other human beings, where the child has the same problem of finding out what effects he can have upon the environment and what effects it can have upon him. The earliest interactions with members of the family may involve needs so strong that they obscure the part played by effectance motivation, but perhaps the example of the well fed baby diligently exploring the several features of his mother's face will serve as a reminder that here, too, there are less urgent moments when learning for its own sake can be given free rein.

In this closing section I have brought together several ideas which bear on the evolutionary significance



of competence and of its motivation. I have sought in this way to deepen the biological roots of the concept and thus help it to attain the stature in the theory of behavior which has not been reached by similar concepts in the past. To me it seems that the most important proving ground for this concept is the effect it may have on our understanding of the development of personality. Does it assist our grasp of early object relations, the reality principle, and the first steps in the development of the ego? Can it be of service in distinguishing the kinds of defense available at different ages and in providing clues to the replacement of primitive defenses by successful adaptive maneuvers? Can it help fill the yawning gap known as the latency period, a time when the mastery of school subjects and other accomplishments claim so large a share of time and energy? Does it bear upon the self and the vicissitudes of self-esteem, and can it enlighten the origins of psychological disorder? Can it make adult motives and interests more intelligible and enable us to rescue the concept of sublimation from the difficulties which even its best friends have recognized? I believe it can be shown that existing explanations of development are not satisfactory and that the addition of the concept of competence cuts certain knots in personality theory. But this is not the subject of the present communication, where the concept is offered much more on the strength of its logical and biological probability.

#### SUMMARY

The main theme of this paper is introduced by showing that there is widespread discontent with theories of motivation built upon primary drives. Signs of this discontent are found

in realms as far apart as animal psychology and psychoanalytic ego psychology. In the former, the commonly recognized primary drives have proved to be inadequate in explaining exploratory behavior, manipulation, and general activity. In the latter, the theory of basic instincts has shown serious shortcomings when it is stretched to account for the development of the effective ego. Workers with animals have attempted to meet their problem by invoking secondary reinforcement and anxiety reduction, or by adding exploration and manipulation to the roster of primary drives. In parallel fashion, psychoanalytic workers have relied upon the concept of neutralization of instinctual energies, have seen anxiety reduction as the central motive in ego development, or have hypothesized new instincts such as mastery. It is argued here that these several explanations are not satisfactory and that a better conceptualization is possible, indeed that it has already been all but made.

In trying to form this conceptualization, it is first pointed out that many of the earlier tenets of primary drive theory have been discredited by recent experimental work. There is no longer any compelling reason to identify either pleasure or reinforcement with drive reduction, or to think of motivation as requiring a source of energy external to the nervous system. This opens the way for considering in their own right those aspects of animal and human behavior in which stimulation and contact with the environment seem to be sought and welcomed, in which raised tension and even mild excitement seem to be cherished, and in which novelty and variety seem to be enjoyed for their own sake. Several reports are cited which bear upon in-



terest in the environment and the rewarding effects of environmental feedback. The latest contribution is that of Woodworth (1958), who makes dealing with the environment the most fundamental element in motivation.

The survey indicates a certain unanimity as to the kinds of behavior that cannot be successfully conceptualized in terms of primary drives. This behavior includes visual exploration, grasping, crawling and walking, attention and perception, language and thinking, exploring novel objects and places, manipulating the surroundings, and producing effective changes in the environment. The thesis is then proposed that all of these behaviors have a common biological significance: they all form part of the process whereby the animal or child learns to interact effectively with his environment. The word *competence* is chosen as suitable to indicate this common property. Further, it is maintained that competence cannot be fully acquired simply through behavior instigated by drives. It receives substantial contributions from activities which, though playful and exploratory in character, at the same time show direction, selectivity, and persistence in interacting with the environment. Such activities in the ultimate service of competence must therefore be conceived to be motivated in their own right. It is proposed to designate this motivation by the term *effectance*, and to characterize the experience produced as a *feeling of efficacy*.

In spite of its sober biological purpose, effectance motivation shows itself most unambiguously in the playful and investigatory behavior of young animals and children. Specimens of such behavior, drawn from Piaget (1952), are analyzed in order to dem-

onstrate their constantly transactional nature. Typically they involve continuous chains of events which include stimulation, cognition, action, effect on the environment, new stimulation, *etc.* They are carried on with considerable persistence and with selective emphasis on parts of the environment which provide changing and interesting feedback in connection with effort expended. Their significance is destroyed if we try to break into the circle arbitrarily and declare that one part of it, such as cognition alone or active effort alone, is the real point, the goal, or the special seat of satisfaction. Effectance motivation must be conceived to involve satisfaction—a feeling of efficacy—in transactions in which behavior has an exploratory, varying, experimental character and produces changes in the stimulus field. Having this character, the behavior leads the organism to find out how the environment can be changed and what consequences flow from these changes.

In higher animals and especially in man, where so little is innately provided and so much has to be learned about dealing with the environment, effectance motivation independent of primary drives can be seen as an arrangement having high adaptive value. Considering the slow rate of learning in infancy and the vast amount that has to be learned before there can be an effective level of interaction with surroundings, young animals and children would simply not learn enough unless they worked pretty steadily at the task between episodes of homeostatic crisis. The association of interest with this "work," making it play and fun, is thus somewhat comparable to the association of sexual pleasure with the biological goal of reproduction. Effectance motivation need not be

conceived as strong in the sense that sex, hunger, and fear are strong when violently aroused. It is moderate but persistent, and in this, too, we can discern a feature that is favorable for adaptation. Strong motivation reinforces learning in a narrow sphere, whereas moderate motivation is more conducive to an exploratory and experimental attitude which leads to competent interactions in general, without reference to an immediate pressing need. Man's huge cortical association areas might have been a suicidal piece of specialization if they had come without a steady, persistent inclination toward interacting with the environment.

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## THE GEOMETRY OF VISUAL SPACE<sup>1</sup>

A. A. SMITH<sup>2</sup>

Defence Research Medical Laboratories, Toronto

In the Luneburg (1947) theory of binocular space perception, the geometry of visual space is demonstrated to be non-Euclidean. The demonstration logically proceeds in two steps. First, from a limited number of mathematical assumptions, it is shown that the visual space must be one of the Riemannian spaces of constant curvature, having either the positive curvature of elliptic space, or the zero curvature of flat Euclidean space, or the negative curvature of hyperbolic space. The most rigorous mathematical treatment of this problem has been published by Blank (1958).

The choice among the three types of space is then made by appeal to special experiments. These experiments are all of the same general character: an observer, confronted with a distribution of luminous points, is asked to modify the initial configuration so as to give himself a specified perception. This perception is customarily one which involves simple notions of equidistance, collinearity, and the like. Blank (1953) gives a comprehensive account of the experimental procedures. The results can be accounted for on the hypothesis that the visual space exhibits a constant negative curvature.

We do not quarrel with this conclusion. Rather, we propose to show that the negative curvature of visual space can be derived entirely by mathematical arguments, given a different set of initial assumptions.

The argument will proceed in two steps: first, a characterization of the visual space under defined conditions of observation; and then a mathematical analysis, originally developed by Eddington (1946, Ch. 1), and modified to fit the visual case.

### VISUAL SPACE

We restrict ourselves to those observational conditions which have been used in experimental tests of the Luneburg hypothesis. In particular, following Blank (1958, p. 328), we consider "only those stimuli consisting entirely of isolated small lights, in appearance like stars, in otherwise completely dark surroundings." These point sources we will call "*p*-particles," and we assume them to be randomly distributed throughout physical space. The set *P* of all possible *p*-particles can be shown to be at most denumerably infinite.

We consider now an observer, situated at some fixed point in physical space, but able to look about freely in all directions. The set *P* will appear to him as a subset *V* of discriminable visual points, or "*v*-particles"; this subset *V* constitutes the visual space of our observer; it is characterized by the following axioms:

I. *There is an irreducible uncertainty in the position of a given v-particle in visual space; this uncertainty will usually be referred to a visual frame of reference and will be spoken of as the "uncertainty of the visual origin."*

II. *The set V of all possible v-particles is finite; i.e., the set V has associated with it a "largest integer,"*

<sup>1</sup> Defence Research Medical Laboratories Project No. 212, DRML Rep. No. 212-9, PCC No. D77-38-50-04, H.R. No. 176.

<sup>2</sup> Now at Assumption University of Windsor, Windsor, Ontario.



$N$ , which we shall call the "number of  $v$ -particles."

III. *The probability distribution of the  $v$ -particles is uniform*; by this we mean that, if two distinct volumes of visual space appear equal in extent to the observer, they will have equal probabilities of containing a  $v$ -particle.

We consider now a very large number,  $N$ , of  $v$ -particles, with uniform probability distribution in visual space. Let  $V_0$  be a randomly chosen volume of visual space, large enough to contain a considerable number of  $v$ -particles. The probability of a particle lying in  $V_0$  we shall write as  $p$ ; the expected number in  $V_0$  is therefore

$$n_0 = Np$$

The actual number will fluctuate about this expected value and, for  $N$  and  $n_0$  large enough, the distribution of

$$y = n - n_0$$

will be normal, with variance

$$\text{var}(y) = n_0(1 - n_0/N) \quad [1]$$

We now set  $x = y/n_0$ , and obtain

$$\text{var}(x) = 1/n_0 - 1/N \quad [2]$$

By Equation 2, we express the fluctuation in the number of particles in a fixed volume of visual space as being due to two independent sources of variation, one of which depends on the sample size ( $n_0$ ), while the other depends on the finiteness of the population ( $N$ ). The two variances are to be combined negatively.

We now look at the particle density, which we may write

$$d = n/V_0$$

The fluctuation arising from the finiteness of  $N$  changes an exact particle density into an uncertain one

$$d = d_0(1 + x)$$

Instead of considering an uncertain number of particles in a fixed volume, we can consider an exact number of particles, and transfer the uncertainty to the containing volume, where

$$d = n/V_0 = n_0/V$$

By putting

$$V = V_0/(1 + e)^2$$

the uncertainty is now contained in a linear scale factor  $1 + e$ . We now wish to transform the distribution function of  $x$  into a function of  $e$ . If the transformation were only over discrete values, the relation would be

$$(1 + x) = (1 + e)^3$$

Since, however, we are dealing with continuous functions, we must insert a factor proportional to  $de/dy$ . The relation is then

$$(1 + x)dx = k(1 + e)^3de$$

which gives on integration

$$(1 + x)^2 = (1 + e)^4$$

Or, on expanding and neglecting squares and higher powers

$$x = 2e$$

Since, by Equation 2, the standard deviation of  $x$  (due to the finiteness of  $N$ ) is  $1/\sqrt{N}$ , the standard deviation of  $e$  is

$$\sigma_e = \frac{1}{2}\sqrt{N} \quad [3]$$

In order to take account of this as an uncertainty in the scale of measurement, we must introduce the uncertain scale  $(1 + e)$  into the visual coordinates.

We consider a  $v$ -particle at a distance  $r$  from the origin. If we assume this distance to be measured, or estimated, with the aid of a "scale" which has a standard error  $\sigma_s$ , the standard error of  $r$  cannot be

less than  $\sigma_e r$ . The total uncertainty in the coordinates of the  $v$ -particle will therefore be compounded of two factors: (a) an uncertainty  $\sigma$  in all directions due to the uncertainty of the visual origin, and (b) an uncertainty  $\sigma_e r$  in the radial direction only, due to the uncertain scale of measurement of  $r$ . Remembering that the scale uncertainty is to be combined negatively with all other sources of variation, the resultant standard deviations are

$$\begin{aligned} \text{radial} &= \sqrt{\sigma^2 - \sigma_e^2 r^2} \quad [4] \\ \text{transverse} &= \sigma \end{aligned}$$

We shall call Equation 4 the local uncertainty of the visual frame of reference. The local uncertainty at a given location in a given direction may be used to define a standard extension which we adopt as the unit for measuring (or estimating) lengths in that direction in that locality. Using this unit, we write the expression for the distance between two infinitely near points, in terms of radial and transverse elements, as

$$ds^2/\sigma^2 = dr^2/(\sigma^2 - \sigma_e^2 r^2) + r^2 d\theta^2/\sigma^2 + r^2 d\theta^2 d\phi^2/\sigma^2$$

which becomes, on simplifying and setting  $K = \sigma_e^2/\sigma^2$ ,

$$ds^2 = dr^2/(1 - Kr^2) + r^2 d\theta^2 + r^2 d\theta^2 d\phi^2 \quad [5]$$

We recognize in Equation 5 a variant of what Weyl (1950) has called the "metrical groundform" of a Riemannian space of constant negative curvature. We have, therefore, arrived at the same conclusion as Luneburg and his followers.

#### DISCUSSION

Starting from a consideration of the probability scatter of particles in an hypothetical visual space, we have

been led to attribute part of this scatter to an uncertainty in the scale by which we measure or estimate distances. By applying the standard deviation  $\sigma_e r$  negatively, we in effect correct the whole random fluctuation of a measured distance  $r$  for the part attributable to the scale uncertainty, so as to obtain what we may consider as the "true" distribution corresponding to an exact standard. This elimination changes the metric so that Euclidean "flat" space becomes transformed into a space of constant negative curvature.

To treat of visual space as non-Euclidean means, then, that *the curvature replaces the scale uncertainty*. This is, for the purpose of describing what occurs in certain classes of experiments, a useful transformation. But there may well be other kinds of visual data where we would do better to represent the observations, without correction, in flat space, and retain an explicit expression for the scale uncertainty.

The question of the nature of visual space may be looked at in a more general way. Weyl (1950) cites the distinction made by Riemann between discrete and continuous manifolds. The measure of a discrete manifold is determined by the number of elements in it; while for a continuous manifold, we must "seek the ground of its metric relations outside it" (p. 97). Physical space, Weyl remarks, is in itself devoid of form; "it acquires definite form only through the advent of the material content filling it and determining its metric relations" (p. 98).

The present approach may be thought of as characterizing visual space *ab initio* either as a discrete manifold of  $N$  elements or as a continuous space whose only content — *under the defined conditions of obser-*

vation—is a finite set of  $N$   $v$ -particles, distributed uniformly through space. In either event, it is not surprising to find the metrical groundform determined by  $N$ , the number of  $v$ -particles.

#### SUMMARY

An analysis of visual space perception is given, in which only stimuli consisting entirely of isolated point-sources in otherwise dark surroundings are considered. The observer is restricted to a fixed point of origin, but is otherwise free to look about in all directions.

Consideration of the probability dispersion of points in the visual field leads to an expression for the uncertainty of the scale of measurement.

A Riemannian space of constant negative curvature arises when this scale uncertainty is eliminated by a mathematical transformation.

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## THEORETICAL NOTES

### COMMENT ON "INTRALIST GENERALIZATION IN PAIRED-ASSOCIATE LEARNING"

WILLIAM F. BATTIG

*University of Virginia*

Murdock (1958) has recently questioned the common principle which states that intralist generalization in paired-associate learning first increases to a maximum and then declines with practice. Instead, he concludes that such generalization is maximal at the start of paired-associate learning and consistently decreases as learning proceeds. Although an impressive array of arguments have been presented in support of this position, Murdock's presentation suffers from two serious defects, with the result that he fails to come to grips with the issue with which he purports to be concerned.

To begin with, Murdock fails to delineate what he means by "amount of intralist generalization." He quotes a definition given by Gibson (1940) which, although not entirely clear on this point, seems to refer to the *absolute* strength of generalized response tendencies, which may or may not be related to strength of correct responses. However, Murdock's analysis is at best applicable to intralist generalization *relative* to strength of correct responses. This distinction is of paramount importance with respect to the issue under consideration, as an increase in absolute strength of generalized responses with practice would typically appear as a decrease relative to the greater corresponding increase in strength of the correct responses. Since his suggested method of measuring intralist generalization is appropriate only for relative strength, no information is provided with respect to the course of change in absolute strength of generalized responses as learning proceeds.

A second implication of Murdock's analysis is that he has in effect defined the problem away on logical grounds and all but eliminated the empirical value of

the concept of intralist generalization in paired-associate learning. He argues that the appropriate method of attacking the question would involve the elimination of "omissions" and "intrusions," so that only the possibility of correct or incorrect (which he equates to "generalized") responses remain. Therefore, any given response must either be correct or a generalized response. It is obvious that a perfect inverse relationship will hold between "intralist generalization" as measured in this way and frequency of correct responses, so that change in generalization with practice will be the mirror image of the curve of paired-associate learning. Intralist generalization thereby becomes nothing more than the absence of correct-response learning. Murdock (1958, p. 308) recognizes this point, but his failure to appreciate its implications is indicated by his subsequent consideration of "related studies," which become largely irrelevant to the question as he has defined it.

In addition, Murdock's conclusion seems curious in view of his argument that intralist generalization cannot take place unless preceded by some correct response learning (1958, p. 306). This almost necessarily implies that such generalization tendencies, when measured in terms of absolute strength, cannot be maximized on the first trial when no learning of correct responses has yet taken place. If intralist generalization is of zero strength at the beginning of learning, and is ever to exceed this level, it obviously cannot be maximized at the start of paired-associate learning.

What Murdock actually has done is to show (a) that paired-associate learning takes place (using an indirect measuring technique) and (b) that the shape of the

learning curve is monotonically increasing. "Intralist generalization" in this sort of formulation represents a kind of "waste-basket" concept with near 100% redundancy. As far as the course of change in absolute strength of intralist generalization with practice is concerned, the answer still awaits the development of a method for measuring this independently of the strength of correct responses, as is done in conditioning ex-

periments. Murdock's analysis has no bearing on this issue.

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## A RE-EXAMINATION OF GENERALIZATION

ELEANOR J. GIBSON

*Cornell University*<sup>1</sup>

In his recent paper, Murdock (1958) argued that generalization in paired-associate learning started at a maximum and steadily decreased as learning proceeded. There are two points I would like to make with respect to the issues which he discussed. One concerns his criterion of generalization in the paired-associate situation. The other concerns the definition of generalization as a psychological concept.

First, what is the criterion of generalization? Murdock produces what he calls maximum generalization at the beginning of learning by telling his *S* to be indiscriminate, by forcing him to guess. Does one really want to admit as a case of generalization a response which *S* knows is wrong and wouldn't have made if he had been told to be as discriminating as possible? Permitting omissions and intrusions, Murdock says, introduces a systematic bias (p. 307). Does not the instruction forcing *S* to respond introduce the opposite bias toward indiscriminate response?

Obviously, in paired-associate learning, overtly measurable stimulus generalization must increase before it can decrease, since *S* has no response tendencies available before the learning begins. Some learning must occur before *S* can say anything at all. If such tendencies are artificially produced before learning starts by giving *S* a list of responses and telling him to start by emitting any of them, right or wrong, the situation is naturally altered. These are the conditions proposed by Murdock, but they do not seem to be the right situation for settling the question of whether generalization increases or decreases with learning.

The multiple-response situation of

paired-associate learning is not the best place to study progressive trends in generalization (though I certainly thought so 20 years ago). Murdock says, "If the chief purpose is to study generalization, the responses are merely a means to an end . . ." (p. 307). In that case, an identification experiment, with only the responses "same" or "different," seems ideal. I used such a situation to study differentiation of forms in children (Gibson & Gibson, 1955), and, as it happened, generalization decreased steadily as practice continued. The *S* was shown a standard stimulus item, and his recognition of it was tested when it was interspersed in a series of items of varying degrees of similarity. The criterion of generalization was simply false recognition of the standard stimulus. The *S* was given every encouragement to be as discriminating as possible.

Instead of arguing about criteria to be applied to a complicated verbal learning situation, which forces one to make predictions dependent on the conditions and rules accepted, it might be better to re-examine the concept of generalization. I would now say that stimulus generalization is simply the fact that a number of stimuli will elicit the same response. When such a group of stimuli is examined, it is found that they are "alike" in one or more respects. These statements imply nothing about progression or a change toward differentiation. It is necessary, for that, to discuss some other aspects of the concept.

Generalization, as many psychologists have pointed out,<sup>2</sup> is of two kinds. Primary generalization occurs in the absence of conceptualization in young animals and in lower organisms, as well as

<sup>2</sup> For example, Hull (1943), Schlosberg and Spolomon (1943), Razran (1949), Woodworth and Schlosberg (1954, p. 755 ff.), Brown (1958, p. 286 ff.).

<sup>1</sup> During 1958-59 at The Institute for Advanced Study, Princeton, N. J.



in human adults. In my opinion, it is essentially an inability to discriminate among a group of stimulus objects. Members of such a group usually appear to the psychologist to be similar, but recognition of similarity actually implies the ability to discriminate. Stimulus equivalence exists, therefore, but not "perceived similarity" for the behaving organism.

Primary generalization does not occur solely in learning situations. A bird which accepts china eggs instead of its own as objects to be incubated provides an example of primary generalization for an instinctive response. Different marks on a piece of paper are confused by a young child, although later he differentiates them as specific letters. It does not make sense to predict a *rising* curve of generalization as frequency of exposure to these marks on paper increases. A lack of specificity in the class of stimuli responded to in a discrete way is characteristic of an early stage of development.

But secondary generalization occurs too, and very frequently in verbal learning situations. It is a result of classifying or categorizing on the part of the organism—an activity in which similarities and differences are both operative. Synonyms, a kind of material often used for learning experiments, are examples of secondary or "mediated" generalization. This kind of generalization obviously increases with learning. For example, Riess (1946), after conditioning an electrodermal response to a word, found a relative increase of generalization to synonyms from 7 years up, but a relative decrease of generalization to homophones. The younger children confused the word with its rhyme far more than with its synonym; but with age and greater knowledge of word meanings, the homophones were differentiated, while the synonyms acquired similarity by becoming members of the same category. The point is that one kind of generalization decreases with age and learning, but the other kind increases.

Where do nonsense syllables or non-

sense forms stand with respect to this distinction? The two kinds of generalization seem to be compounded in them. They are purposely made to be hard to discriminate. But they are made up of already discriminated elements. Many Ss report that they discriminate them by putting them into distinct categories (*e.g.*, a friend's initials for the nonsense syllable, or the image of a kite for the nonsense form). When several are placed in the same category, more confusion may result until a further separation is achieved. Experiments on pre-differentiation attempt to get at this process by comparing the effectiveness of methods of producing differentiation (Arnoult, 1957). So far, no general conclusion can be drawn about the process. Perhaps the divergence of results is explained by the difference in stimuli employed (nonsense forms, syllables, fingerprints, lights with differing orientations, *etc.*). But these experiments have an advantage over paired-associate learning for studying the course of generalization and discrimination, since the final criterion may be independent of learning a set of correct responses. Some more direct measure of differentiation, such as a same-different judgment, can be used; or a transfer criterion, which takes account of the role of the responses by the use of suitable control groups.

To summarize, a definition of generalization need not depend on learning, though traditionally it did. It is suggested that a distinction be drawn between primary generalization, defining it as inability to discriminate, and secondary generalization which results from conceptualization or something simpler but analogous to it. Prediction of increase or decrease of generalization in any learning task would depend on the kind of generalization involved, as well as the criterion selected for measurement. Examination of experimental procedures is essential for useful criticism, but so also is clarification of basic psychological assumptions.

Many problems about generalization remain unsolved. How the gradient of

generalization is related to the distinctions drawn above, and what explanatory "power," if any, the concept retains are two of the issues to be considered. The author hopes to contribute further to a reformulation of the whole problem, and no doubt others will, too.

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## REMARKS ON "INTRALIST GENERALIZATION IN PAIRED-ASSOCIATE LEARNING"

WILLARD N. RUNQUIST<sup>1</sup>

*Hobart College*

In a recent article, Murdock (1958) has questioned the usefulness of the principle that during the course of paired-associate learning, generalization first increases then decreases (Gibson, 1940). It is contended here that not only are there defects in Murdock's approach to the problem, but the data which he presents are largely irrelevant to the argument. Since the logical and methodological problems of Murdock's approach have been adequately handled elsewhere (Battig, 1959; Gibson, 1959), this comment will be restricted to an analysis of some of the data presented by Murdock as a criticism of the principle of increasing and decreasing generalization.

Murdock contends that data of paired-associate learning purporting to show a noticeable increase in the frequency of intralist errors before these errors decrease in frequency are equivocal. However, it has never been clear to the present author why the amount of generalization (or perhaps to use a less ambiguous term, the strength of interfering responses due to generalization) and number of overt errors should be expected to be perfectly correlated. The occurrence or nonoccurrence of any error is governed by many factors other than the absolute strength of this interfering response or even the relative strength of the correct and interfering responses. Under ordinary conditions of learning, *S* is given a limited amount of time to respond to each stimulus in the list. The presence of a strong interfering response tendency is more often indicated by an omission or failure to respond than the overt occurrence of a wrong response. It has been demonstrated empirically in retroactive inhibition and associative interference experi-

ments that the more sensitive index of amount of interference is provided by omissions (Underwood, 1945; Spiker & Holton, 1958). In fact, overt intrusions obtained in these experiments are not a monotonic increasing function of the degree of learning and, presumably the strength of directly interfering response tendencies at all. Since generalized responses are essentially interfering responses also, one might expect the same lack of correspondence between their strength and the number of overt errors in the case of intralist interference, as is obtained with directly competing responses between lists. Thus, it is contended here that intralist errors may be an inadequate measure of generalization, both under the special experimental conditions specified by Murdock and in the more traditional kind of experiment with a fixed anticipation interval and no previous response learning.

Since the bulk of the data presented by Murdock concerns these overt errors, it is felt that it is irrelevant to the question, since the hypothesis concerning the course of generalization need not imply an increase and decrease in overt errors. This does not mean that omissions are a measure of the absolute strength of generalized responses. As Battig has pointed out, there is no way of measuring this interference directly. Unless a test of Gibson's hypothesis can be made in some indirect fashion, the empirical solution to the problem of the course of the strength of generalization tendencies cannot be attained at present.

Murdock has attempted an indirect solution by deducing from Gibson's hypothesis that some predifferentiation training (PD) will result in negative transfer if the PD is carried to the point where generalization is at a maximum. Just how he arrives at this prediction is

<sup>1</sup> Now at Pomona College.

not clear. If generalization must increase before it decreases, then positive transfer should always result from any PD training. When PD training is carried to the point of maximum generalization before paired-associate learning is begun, generalization need only decrease from that point, whereas if no PD training has been given, generalization must increase before it decreases. Thus, using trials to some criterion as a measure of transfer, any PD training should be beneficial.

Murdock is not specific as to how negative transfer is to be measured, even though this is of primary importance. If PD training were to produce any evidence of increased interference, this would have to occur on the first few trials before generalization has begun to decrease. Then as generalization begins to decrease, the interference should dissipate; and comparing performance to that when no PD was given, facilitation should then result. Most of the data cited by Murdock, however, is not presented by trials. Furthermore, most of the studies fail to control for nonspecific factors produced by PD training such as learning-to-learn and warm-up. In general, then, these data cannot be considered a test of the hypothesis.

One further point might be made. It

is not necessary to assume that PD training results in an increase in generalization before there is a decrease. The increase in strength of generalization *tendencies* (strength of competing responses) described by Gibson (1940) appears to be partially a result of the growth of response strength of the interfering response to its correct stimulus. It is this response strength which generalizes to similar stimuli and produces a "generalization tendency." The PD process need not involve a mechanism of this kind; hence, it could be monotonically decreasing. It is felt that this is an empirical question, however, upon which there are no adequate data.

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## A REPLY TO BATTIG, GIBSON, AND RUNQUIST<sup>1</sup>

BENNET B. MURDOCK, JR.

*University of Vermont*

Whether generalization increases or decreases during paired-associate learning obviously depends upon the definition of generalization and the conditions under which it is to be studied. In my previous article (Murdock, 1958) the definition of generalization referred to the absolute, not the relative, amount of generalization, and it was assumed that the amount of generalization would be measured by the number of generalized responses. Also, on the assumption that in the rote learning of nonsense syllables there is a minimum of conceptualization, I was referring to primary and not secondary generalization. If it is agreed that in the conventional paired-associate learning task the generalization is primary and not secondary, then it would appear that Gibson essentially agrees with the position previously stated; as she says, "It does not make sense to predict a rising curve of generalization. . . ."

However, no such agreement is evident in respect to the appropriate conditions for paired-associate learning. The difficulty with the standard type of paired-associate learning task is that *S* must learn both the responses and the pairings, and from the data it is impossible to separate these two different processes. The solution that I suggested to this problem was to tell *S* in the instructions what the responses are (or else use responses with which *S* is already familiar), then force him to respond from the first trial on. Forced-choice techniques are common in threshold studies and in psychological testing, and it has been shown (Adams, 1957) that there are situations in which *S* can respond with above-chance accuracy even though he reports complete lack of confidence

(i.e., that he is "merely guessing"). Prohibiting intrusions and omissions modifies the traditional paired-associate procedure, but the modification would seem to be in the direction of making the task simpler and easier to analyze. Whether these conditions are "appropriate" or not clearly depends upon the purpose of the study, but as I tried to show by means of a logical analysis under these conditions generalization can only decrease with practice.

To answer several specific points from the preceding articles: (a) Battig raises the question as to how generalization tendencies can be maximized on the first trial when no learning of correct responses has taken place. The answer is that the responses should be specified in the instructions so *S* will have learned the responses prior to the first trial; then generalization will be maximal in the first trial. If you don't tell *S* what the responses are, you can't even make any statements about generalization in the first few trials because *S* doesn't know the responses, and you obviously can't have generalization (overt errors) before there are responses to generalize. (b) Under the suggested conditions, generalized responses and correct responses are not so much redundant as complementary (as, for instance, amount forgotten and amount retained). I recognized this point in the original article (first new paragraph, p. 308), and this is why no new data was presented—it would have been redundant. (c) Runquist points out that, in the traditional paired-associate procedure, amount of generalization and number of overt errors are not perfectly correlated. It was for exactly this reason that, in the section entitled "Related Studies," I concluded that the studies were not conclusive. Although these studies are inconclusive, it is a little hard to see how

<sup>1</sup> I would like to thank W. F. Battig, E. J. Gibson, and W. N. Runquist for advance copies of their manuscripts.

they are "irrelevant." (d) The transfer deduction was not intended as an indirect solution but rather as one way of testing the two opposing theories. It was suggested that, if generalization initially increased, negative transfer should result because at the start of the second task learning the "predifferentiated" list would in effect be at a higher level of intralist similarity than the control list. If the transfer were a function of changes in generalization and if changes in generalization during the second task merely started where the first task left off, then Runquist would be correct; with most measures of transfer any PD training should lead to positive transfer.

Finally, I would like to point out again that omissions and intrusions introduce

a systematic bias and that some method (such as a forced-choice technique) must be used to circumvent this difficulty. When this is done, overt errors become an appropriate measure of primary generalization, and under these conditions clearly generalization can only decrease with practice in paired-associate learning.

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*Director of Graduate Training in Clinical Psychology and Clinical Services, Purdue University*

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